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GEO. L. ENGLISH'S ANNOUNCEMENTS.

Mr. ATKINSON of our firm sailed on the Etruria, May 16th, for an extended collecting tour through Europe. We hope to announce next month some of the many important additions to our stock which we expect as the result of his trip.

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THE

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[THIRD SERIES.]

ART. LIII.—*The Study of the Earth's Figure by means of the Pendulum;* by E. D. PRESTON.

[Read before the Brooklyn Institute Feb. 26, 1891. Published by permission of the Superintendent of the U. S. Coast and Geodetic Survey.]

History.

THE idea of finding the size and shape of the earth is probably one of the oldest in the history of science. Each age has added to the knowledge of the age before it, and each one has by its additions to existing data contributed to the solution of the problem. From the time of Anaximander 600 years before Christ, when it was thought to be a cylinder with a height equal to three times its diameter, down to the last deductions of Clarke and Bessel which point to a spheroid with three unequal axes, successive theories have been tested by physical observations and corrected or modified by the facts revealed by experience. It is not worth while to review all the ancients thought or did on this subject. Such a study would be interesting but not profitable for the present purpose. The turning points or mile-stones on this highway of inquiry may, however, be noted as showing how slow has been the progress towards what we now believe to be the truth. The cylindrical theory supposed the land and water to be on the upper base. Seven successive generations accepted this idea and when it was no longer considered tenable a cube was substituted for the cylinder. What a striking difference between the intellectual activities of an age that required several hundred years to pass from a cylinder to a cube, and was satisfied with this conclusion, and an age that in one-half the time has determined the distance of the heavenly bodies and studied

their composition. Aristotle, three centuries before Christ, supposed the earth to be spherical, and Eratosthenes 100 years later was the first to actually compute its dimensions from observations of the sun's shadow. Nothing of course was done in this direction in Europe during the dark ages. With the revival of learning in the 15th century the spherical theory again took shape and during the 16th (1525) Fernel determined the earth's dimensions essentially as it is done to-day; that is, by measuring the distance between two points and observing their difference of latitude. From this time on, it being admitted that the shape of the earth was something like a globe, the question was and still is, how much does the surface depart from that of a perfect sphere, and what is its actual size. In 1669 Picard measured the length of a degree by means of triangulation. This was a long stride in advance of all previous work, because here for the first time spider lines were used to mark the optical axis of the telescope. Newton used his value in the proof that the moon falls toward the earth in obedience to the law of universal gravitation. A score of years later Cassini greatly extended the measurement of arcs in France, but from some unfortunate circumstance his results were contrary to the Newtonian theory, and also to what had come to light a few years before, namely, that a pendulum vibrates much slower at the equator than in middle and higher latitudes. This gave rise to a controversy which brought about the famous work of the French academicians in Lapland and in Peru. Their labors effectually closed the question of the relative lengths of the polar and equatorial axes, and since then we have simply been making closer and closer approximations to the still unknown truth. From the accumulation of refined observations other knowledge than that directly sought has come to light. It is found that an ellipsoid better fits the observations than a spheroid, and there seem to be physical reasons why the northern and southern hemispheres should not be exactly equal. Moreover, the actual surface of the earth departs everywhere from the mean figure adopted in all theoretical computations, and it is generally admitted that this mean figure cannot be corrected until we know something more about the actual figure.

The quantities involved.

Now to recount: first we had the cylinder, then the cube, then the sphere with its variations into spheroid, ellipsoid and geoid. There is where we are at present; and what I shall have to say will be touching the instruments and methods by which the eccentricity, one element in the earth's figure, is

determined. Let us first understand what kind of quantities we are dealing with. The difference between two radii of the earth, one being polar and the other equatorial, is about thirteen miles. This bears the same relation to the whole radius that one inch bears to twenty-five feet. So that had we a model of the earth in its true proportions it would be quite impossible to see with the naked eye whether it was flattened at the poles or not. The first practical demonstration of a change of the force of gravity with a change of latitude was had a little over 200 years ago when a clock was carried from France to Guiana. This clock kept accurate time in Paris but lost two minutes per day in Cayenne. The pendulum had to be shortened about $\frac{1}{10}$ of an inch in order to make it beat seconds, as it had done in a more northern latitude. It was thus seen that the pendulum could be used to measure the force of gravity. The change in the time of one oscillation over limited areas is, however, very small; one mile in distance making a difference of one two-millionth of a second in the time of vibration; or stated in another way, a pendulum thirty-nine inches long that beats seconds at the equator would only have to be lengthened by $\frac{1}{5}$ of an inch to make it beat seconds at the pole. When we consider that one-quarter of the entire circumference of the earth only changes the length of the second pendulum by its $\frac{1}{200}$ part, it is evident that a change of latitude even for a country as large as the United States affects the pendulum by what may be called a minute quantity. Then the force of gravity changes with the elevation; but our highest mountains only alter the time of oscillation by $\frac{1}{1500}$ part when distance alone is considered, and the effect is even less than that if the attraction of the mountain is taken into account. Of this we shall speak later. It is thus seen that in all work pertaining to the measurement of the force of gravity we are obliged to deal with very small quantities and that methods must be devised delicate enough to appreciate them. How far these have been successful may be judged from the fact that independent determinations of the time of an oscillation do not differ as much as the one hundred thousandth part of a second. It is not asserted that differential gravity is always known to this degree of accuracy, but simply that there is no difficulty in making the pendulum repeat itself with no greater error than that mentioned. When we come to measure the absolute force of gravity, besides determining an interval of time, we are required to measure an interval of space. This can be accomplished with a degree of precision far exceeding that attained in the time measures. Not only is it much easier to obtain the one hundred thousandth part of a meter than to get the corresponding fractional part of a second, but the effect

of an error in the time of an oscillation is doubled when it is referred to the length of the pendulum; so that the weakest part of the entire investigation is the length of the time of one oscillation. For this reason it has been assumed that the pendulum can never be made to compete with metal bars in giving us a uniform standard of length. Whether it is better to refer our standards to the wave-length of light, or to some material thing involving permanence of capacity, is not in the exact line of our thought at present. Objections have been urged against both these methods. The former because it was supposed the earth might eventually move to a region of space where the wave-length of light was different, and the latter because a capacity-measure may change its three dimensions unequally so that the permanence of its capacity would be no proof of the permanence of one of its linear dimensions. What most concerns us is to know that the seconds pendulum is not our best standard of length.

Method of Study.

The most advantageous way of treating the figure of the earth is now conceived to be different from that employed hitherto. The measurement of an arc of the meridian by triangulation is not the best means of arriving at the flattening although it gives the actual size of our planet with an accuracy fully equal to the requirements of the case. The problem should be separated into two distinct investigations, and to each one of them methods should be applied, that will determine the unknown quantities to the best advantage. Let pendulum observations and the moon's parallax determine the flattening and let triangulation measure the actual size. Then we shall have each method working in a field where it has the greater power and the results will be correspondingly better. When the flattening is determined its value may be used as a known quantity in the equations that determine the length of the axes. This method is suggested by Professor Harkness. Admitting then that pendulum observations must be employed for the study of the earth's figure, what is the best way of doing it? Here we are confronted by two distinct schools. The Germans, followed by the Russians and Swiss, have always favored absolute determinations of gravity. The English stand as the exponent of the rival school and only measure differentially. The U. S. Coast and Geodetic Survey has followed both methods to some extent, and now seems to be favoring differential measures especially for a preliminary survey of the country. The difference between the methods is briefly this: the Germans measure the actual force of gravity

at every station. This requires an accurate determination of the length of the pendulum as well as the time of oscillation, and also necessitates the measure of the vibratory movement of the stand on which the pendulum swings. The English content themselves with relative measures from one station to another and simply compare the forces of gravity, by counting the number of oscillations made by the same pendulum in the two places. They determine absolute gravity only at a base station. It is evident that the latter method is immeasurably superior in point of economy, and we may say that it can be made to yield results fully equal in accuracy to those of its rival school. Moreover the differential method has the great advantage of eliminating all those sources of error that are practically the same for each station.

General Results of Pendulum Work.

Knowing now which method best suits our purpose a still further question arises. How accurately shall we do the work? Certainly not as accurately as we can. That would be bad economy from every point of view. Nothing is gained by measuring the force of gravity to its 1/100000th part to determine local deflections when these variations themselves are several times as great. No fact is more certainly demonstrated than that certain places on the earth's surface present variations of the force of gravity quite exceeding anything to be expected either from the amount or density of the adjacent matter. These anomalies in many cases baffle all attempts to classify them, but one general result seems to be that mountains are light and islands are heavy. Some of the first, if not the very first pendulum observations made, gave the strange result that the Andes in Equador are not much heavier than water. Foster's celebrated series on Green Mountain gave a result indicating that this volcanic formation is about twice as heavy as cork. The sacred mountain in Japan has given a similar result, namely, that the mountain is lighter than would be indicated by its volume and density. Haleakala in the Hawaiian Islands seems to be of the same mean density as the rocks on its surface. One observer has even gone as far as to say that the Alleghany Mountains in Pennsylvania weigh less than nothing, meaning by this, that if gravity at the summit be corrected for elevation, the result is not more than gravity at the base, showing the downward attraction of the mountain to be practically insignificant. If we glance at a few island stations, gravity is found to be mostly in excess of what it ought to be. The most striking examples are Fernando, St. Helena, Ascension, Minecoy, Isle of France, Bonin, Maui and

Caroline Island. At all of these gravity is more than theory demands, and they are generally islands situated in a deep sea and considerably removed from continental masses. This excess has in many cases been shown to be more than can be due to the extra attraction of the water, so that there must be a real increase in density under the ocean bed. We may therefore look to pendulum observations for information in regard to the internal structure of the earth. Deviations from the law of uniform density may be greater than has hitherto been supposed. Two well known authorities on the subject have already expressed the belief that the center of gravity of the earth does not lie at the center of figure, but is to be found inside the hemisphere that is under the Pacific Ocean. We have an example of this excessive seaward attraction in India. When a chain of triangles was thrown across the peninsula of Hindoostan from the Bay of Bengal to the Arabian Sea, it was expected that the astronomical amplitude of the arc would exceed the geodetic amplitude: in other words that that the plumb-line would be deflected towards the high table lands over which the arc was measured. The contrary was found to be the case. The seaward attraction was more than that from the continent, and an identical result followed from two independent arcs. We may therefore accept the fact as proven, that the attraction of continental masses is in some way partially compensated by a deficiency of density in the immediately underlying strata. It may be asked what relation this defect of gravity on high table lands, as revealed by the pendulum, bears to the horizontal attraction as brought out by a comparison of astronomical and geodetic latitudes. The Himalayan attraction on the plumb-line at Dehra, a point less than 50 miles distant and with an elevation of 2000 feet, is 1/5000th part of the total force of gravity at the earth's surface, whereas the defect of gravity at or near the summit is 1/2000th part of the total force. This somewhat strange result may be explained in two ways. First the deflection at Dehra may be produced by matter lying between this place and the summit; or the great plateau of Thibet with an average elevation of 15,000 feet may exert the attractive influence on Dehra, and the strata immediately under the pendulum station at the summit may possess a very small density. This last view would seem to be supported by the fact that there is a deflection in azimuth as well as in latitude at Dehra.

The condensation theory assumes that all pyramids of matter having their vertices at the center of the earth and having equal bases, contain equal amounts of matter, and that the vertical attraction at any point on an elevated plateau is equal to that obtaining at a point on the sea-level immediately under it.

if we imagine the plateau to be compressed down to that level. An equipotential surface would be several hundred meters below the surface of the actual ocean, and as much above the mean continental surface.

The Earth's Geometrical Figure.

It is probably demanding too much at present, to ask the acceptance of the tetrahedral theory of the earth's figure, but in connection with gravity work allow me to call attention to some points of the argument. It is well known that gravity is in excess at island stations. If we admit the tetrahedral system, these ocean areas are really nearer the center of the earth, and hence should show increased gravity while the continental masses would tend to increase the effect still further by elevating the surface of the sea in their immediate vicinity. It has been shown that the attraction of the Himalayas would elevate the surface of the ocean immediately under them by nearly 1000 feet. This would be equivalent to increasing the distance from the earth's center by 1/16,000th part of itself and gravity would be diminished by twice this amount, which is a very appreciable quantity. Besides nothing is more in accordance with the action of physical laws than that the earth is contracting in approximately a tetrahedral form. Given a collapsing homogeneous spherical envelope, it will assume that regular shape which most readily disposes of the excess of its surfaces dimensions, or in other words the shape that most easily relieves the tangential strains; for while the sphere is of all geometrical bodies, the one with a minimum surface for a given capacity, the tetrahedron gives a maximum surface for the same condition. Experiments on iron tubes, on gas bubbles rising in water and on rubber balloons, all tend to bear out the assumption that a homogeneous sphere tends to contract into a tetrahedron.

These ideas regarding the shrinking nucleus of our globe and the consequent form assumed by the surface are not by any means new. They have long since been formulated by Green and have found favor in France. Mr. Green has even gone so far as to study the land and water areas of the globe, and has succeeded in finding a close correspondence between the actual features and those required by the theory. Africa and Europe are considered as one continent, and a depression is assumed between Europe and Asia. In point of fact there was a time when a glacial sea existed along the Siberian frontier and communicated with the Caspian waters. Admitting also a polar sea and an antarctic continent, both of which seem highly probable; there seem to be reasons for the acceptance

of the theory. Then again, it seems quite well established that our present continental forms are very old, which would indicate that whatever form the contracting earth may be taking, it has been gradually settling into this shape for many millions of years. No reference is made at present to changes on the earth's surface, consequent upon outside conditions. The changes in the eccentricity of the orbit which has its greatest value at intervals of about 2,500,000 years certainly produce vast changes in the distribution of matter, from the accumulation of ice and snow, and from the shifting of the ocean currents; but these are purely surface phenomena, and do not probably affect the permanent shape of the contracting nucleus. Besides, any change produced at a time when the eccentricity was at a maximum, would be counterbalanced when the next minimum occurred somewhat more than a million years later. But the earth's contraction goes on indefinitely throughout all time.

This brings us to a consideration of

The Earth's mean density.

The pendulum has recently been employed in such determination.

It is well known that during the early part of this century Dr. Hutton conceived the idea of determining this constant by comparing the attraction of the earth with that of a mountain of known dimensions. The method of course rests on the assumption that the volume of the mountain is a determinable quantity and also that the mountain is solid. Latitude observations were made on the north and south flanks and the results were compared with the actual differences of latitude obtained by connecting the points by triangulation. This way of getting the earth's mean density has been employed in a number of cases since Dr. Hutton's time and always with approximately the same result. It is evident now that if we have a means of getting a value for the density of the mountain that is independent of the latitude observations we get a rigorous check on the final result. This modification of the problem was applied with entire success in the Sandwich Islands in 1887; and not only was the mountain much larger and higher than in Dr. Hutton's work, but its form was much more accurately known. The entire island of Maui rises to an elevation of 10,000 feet and has on its summit the crater of Haleakala, which is twenty miles in circumference and half a mile deep The whole mountain has been contoured from the sea to the summit giving differences of elevation for every 500 feet. This gave a means of calculating with a high degree of

precision the disturbing effect of this huge mass of lava on a plumb line suspended north or south of it. But the mass of the mountain enters as one of the unknown quantities. This quantity was determined by measuring the force of gravity at the sea-level and at the highest practicable point by means of the pendulum. Knowing the mass of the mountain and therefore its mean density, its attraction at any given point is easily deduced. Now the result of the work was this: the pendulum observations showed that the mean density of the mountain was very nearly one-half that of the earth's mean density, that is, that the island is a little more than two and one half times as heavy as water. This value would lead us to expect, at the point selected on the south shore, a disturbing effect on our star observations of 28″. When the two points on the north and south sides of the island were connected by triangulation a discrepancy of 29″ was brought out. The agreement between the results obtained by two independent methods is so close as to give us considerable confidence in the astronomical and geodetic parts of the work as well as in the measurement of the force of gravity at the upper and lower station. Besides this, rock specimens were secured from many parts of the island at different elevations. Their densities were carefully determined at the bureau of weights and measures in the Coast Survey Office. When a mean value was taken we arrived at the result that the mean density of the mountain is somewhat more than that of the rocks found on its surface. This is contrary to the result generally obtained on mountains and high table lands; and it is notably in opposition to determinations on continental mountains. But let us remember in this connection that the sea level in the neighborhood of continents may be considerably disturbed by the attraction of the land, and that a single mountain in the middle of a deep sea would have practically no influence in elevating the surface of the ocean. In point of fact the island of Maui could not elevate the surface of sea around it by more than ten feet—a quantity easily neglected in this investigation.

One word about the correction for

Continental attraction.

We know that on a sphere at rest attraction varies inversely as the square of the distance from the center, but in the case of a rotating spheroid this assertion is not true. The actual diminution of gravity from the pole to the equator is about $\frac{1}{200}$th part of itself—this is in part due to the centrifugal force in consequence of the earth's rotation and in part to the spheroidal shape into which the earth has been thrown by this rotation. Bouguer was the first to call attention to the fact

that besides the influences just mentioned some allowance should be made for the matter lying above the sea level; and his formula, based on the relation between the mean density of the earth and that of the crust, is still employed. The propriety of this method of treatment has, however, several times been questioned. In the first place, because observation seems to show mountains and table lands to be much too light, and in the next place, because the excess of gravity noticed at island stations is very nearly accounted for by making a correction for the downward attraction of the sea. Of course we meet with many anomalies in gravity determinations, but it would seem better in the present stage of the subject to make some disposal of the influence of the continents. The estimate may indeed be a rough one, afterwards to be modified as more data accumulates, but in the light of our present knowledge we may apply the corrections as follows: at a station say 1900 feet above sea level a seconds pendulum will lose eight seconds daily on account of its elevation, and it will be accelerated in the same time by three seconds from the influence of the mountain matter, so that both effects together would cause a loss of five seconds per day. This rule of course does not hold strictly at great elevations; nevertheless it was found approximately true in the Sandwich Islands where the pendulum was carried to an elevation of about 10,000 feet. Here we had a daily loss of 41^s from elevation and an acceleration of 13^s from the mountain attraction giving a total loss of 28^s daily. This agrees tolerably with the ratio above stated.

Changes of Latitude.

Closely connected with the variations of the force of gravity are the changes of terrestrial latitudes. Whether the earth's crust is floating on the plastic or semi-plastic nucleus, and really shifts its position with reference to the axis of rotation, or whether latitudes change by reason of the moving of quantities of water and air, it is now impossible to say. Within the last year it has been abundantly demonstrated that latitudes may have an annual variation of a considerable fraction of a second. The International Geodetic Association of Europe has taken the matter up and will send an observer to Honolulu in order that simultaneous observations may be made on opposite sides of the earth. The U. S. Coast and Geodetic Survey has been asked to coöperate and will also send an officer to the Sandwich Islands, besides observing continuously at Washington, San Francisco and other points. The Royal Observatory at the Cape of Good Hope will engage in the work and probably other permanent stations may be established in the southern hemisphere.

The outcome of all this will be that when observations from opposite sides of the earth are compared we shall be able to decide whether the axis of the earth actually shifts its position, or whether changes of latitudes are due to transfers of molten matter below the crust.

If the results at Berlin and Honolulu show opposite phases at the same time, we should expect the latitude to be stationary at Washington, because this point is one-quarter way around the globe, or midway between the other two stations. The conclusion from this would be that there is a real motion of the pole and not a transfer of material inside the earth.

There is a decided maximum and minimum within twelve months with a larger maximum and minimum in a five-year period. In addition to this the Greenwich observations show a long period of inequality extending over sixty years. The cause of the short period movements has been ascribed to the interference of the motion of the axis of inertia with that of the ten-monthly period of the axis of rotation. It can certainly be assumed that the sun and moon produce atmospheric tidal effects changing with the seasons, and it is also known that the shifting of a mass of water covering $\frac{1}{10}$ the earth's surface and being 0·10 meter thick would cause the axis to move 0″·16 —a quantity which is quite measurable in all latitude work of precision. As this depth of water corresponds to an atmospheric pressure of about 0·007 meters it is evident that extensive changes in the density of the air may produce a slight change in the position of the earth's axis; so that it would seem well worth while to measure the force of gravity from time to time at the same place in order to detect changes that would most probably be produced by changes of latitude.

Practical Methods.

When we come to the actual field work, again several methods present themselves for our consideration. The ultimate end of the observations is to find how long it takes the pendulum to make an oscillation at a given temperature and atmospheric pressure. Most of the slight corrections necessary to reduce the different experiments to the same conditions, and thus make them comparable, may for our present purpose be passed over in silence. The reduction to an infinitely small arc involving only simple geometrical considerations is easily disposed of. The influence of the temperature and density of the air requires special treatment, and has been made the subject of careful study by all pendulum observers. The air not only has a buoyant effect on the oscillating body, but by reason of its viscosity adheres to the pendulum and is drawn along

after it. The atmospheric effect has been treated in one system of equations, where the unknown quantities vary directly as certain powers of the pressure and inversely as powers of the temperature. All these corrections are of much more importance in absolute determinations than in relative ones. This brings us to the different methods of observing.

Two methods have been chiefly followed. First by noting coincidences between the gravity or experimental pendulum, and the pendulum of a clock set up a short distance away. This is by far the most easy and accurate method of getting the length of one oscillation of the gravity pendulum. The second method is by registering on a chronograph the passage of the pendulum across a fixed point of reference. Forty of these transits suffice to give a mean value, which carries the accuracy of this part of the operation far beyond that attained in deducing some of the other necessary corrections. The probable error of the mean of a chronographic set is only 0·003 of a second and when this is divided by 15000, the number of oscillations in one swing, we get an accuracy beyond one millionth of a second. This is all that can be desired, but the method of coincidences is still more accurate while it is much less difficult to observe. We may commit an error of many seconds in the time of a coincidence without vitiating the result. The distinctive feature of the last method is this: when we commit an error of one second in noting the time, we do not change the value of one oscillation in the ratio of this error to the length of the swing, because both pendulums are moving along together. An error in the time of coincidence only means that the result will be in error by an amount equal to the ratio, one has gained on the other in the short time between the true coincidence and the one noted, multiplied by the ratio of the error to the whole period. To illustrate by a special case, suppose that in 600 oscillations of the clock pendulum, the gravity pendulum loses two oscillations, and suppose that the coincidence was erroneously noted after 602 oscillations had been made instead of 600. This error is 1/300th of the interval, but far from introducing an error of 1/300 in the length of one oscillation, the error is only 1/300th of the ratio of the gain of one pendulum on the other, that is 1/300 of 1/300 or say 0·00001. It is thus seen that the accuracy of the result is a function of the length of time between two coincidences, and that the longer the interval the more accurate will the result be given. One might suppose therefore that the coincidence period might be indefinitely long, but there are economic considerations bearing on the question. For instance we cannot afford to wait very long for the coincidence because this would entail too much loss of time. Therefore in

general intervals should be chosen which are not longer than are necessary to secure the desired accuracy, and the swing should be repeated in order to eliminate accidental errors.

In the chronographic method, an error in the determination of the interval between the first and last observation is simply divided by the number of oscillations in the interval, and therefore affects the result much more than by the coincidence method.

There have been many ways devised for noting these coincidences. I shall first call attention to a few of the older methods which leave nothing to be desired as to accuracy, but which have been superseded by an elegant arrangement devised by Professor Mendenhall and which, while giving all the accuracy needed makes the observation both simple and easy. First we had a piece of card-board fastened to the clock pendulum. This card-board had on its surface a number of spots which were seen to disappear at each coincidence of phase in the motion. The time of disappearance and reappearance were noted and the mean taken for the true coincidence. For increased accuracy a number of spots were observed, and for convenience in taking the time the spots were arranged in the form of a curve, resembling a hyperbolic spiral, which would give about equal times between successive disappearances for all amplitudes of oscillation. This method was modified in the case of the Peirce pendulums by placing a scale on the clock pendulum and a small needle point on the gravity one. This last procedure is in direct violation of the whole theory of differential gravity measures, namely, that the pendulum must undergo no change from one station to another. However, as the mass added is exceedingly small, and besides is placed very near the center of oscillation, where theoretically it would have no influence whatever on the time of vibration, the method is considered admissible.

Both the preceding ways are applicable only to two moving pendulums and suppose them to be of equal length. The methods depend on sight alone. Coincidences have also been observed by the eye and ear method, by comparing the beat of an ordinary sounder used in telegraphing, with the transit of a pendulum across the vertical thread of a telescope. This avoids carrying a clock from station to station, a break circuit chronometer furnishing the beat every second. The method has not been very extensively employed, as it requires considerable practice on the part of the observer.

We now come to the last way, and which is believed to be in many respects the best: and here we have to do, not with two pendulums, beating approximate seconds, but with a single pendulum beating half seconds, whose coincidence must be

noted with a chronometer beating whole seconds. This required the invention of an entirely new kind of apparatus. It was evident some means must be had by which the coincidence could be noted optically. For this purpose the chronometer was made to open every second the armature of a relay to which was attached an upright thin piece of metal. This metal was perforated by a thin slit which by the movement of the armature passed before a fixed slit a short distance in front of it, so that a light suitably disposed gave a flash every time the chronometer broke the electric current. The apparatus was placed about ten feet from the pendulum, and was so adjusted that the beam of light from the slit fell on two mirrors, one of which was on the pendulum and one near by it. The former was therefore movable by the motion of the pendulum while the latter was stationary. From these mirrors the beam of light was reflected back into the observer's telescope. When the pendulum was at rest, the observer saw two illuminated slits every second in the field of view, but with the pendulum in motion the flash that came from the pendulum mirror could only be seen when the pendulum happened to be near its equilibrium point. It is evident that if the pendulum makes exactly two oscillations for every second of the chronometer, the relation of the images will not change, and we shall see a double flash every second in the middle of the field of view. But if the time of oscillation of the pendulum is slightly different from a half second, it will not return quite to its former position by the time the next flash occurs, and we shall have its image displaced with reference to the one from the fixed mirror. Here then we have two necessary conditions for the appearance of the flash from the pendulum. First the chronometer must open the slit, and second, the pendulum at this instant, which has a duration of about 1/100th of a second, must be in such a position that the image of the slit will be reflected back into the observer's telescope. We only need now a point of reference to which the motion of the movable image may be referred. This is furnished by the flash reflected back from the stationary mirror, and as the image from the pendulum is seen to pass slowly across the field of view, the time is noted when the two images coincide. For pendulum A, the time of oscillation exceeded half a second by 0·0066 seconds, which gave a coincidence interval of $6^m\ 15^s$. This was adopted for the other pendulums. Inasmuch as it was contemplated to use these instruments in all parts of the United States and at all altitudes, it was necessary to consider the effect of a change of latitude and elevation on the coincidence interval, because a period might be chosen at Washington, which would make the intervals too long for conven-

ience on the Canada frontier and too short for accuracy on the Gulf of Mexico. A compromise was made between the several conditions and the above mentioned interval of $6^m\ 15^s$ chosen. It so happens that the effect on the coincidence period is about the same, whether we pass from the latitude of Washington to the Gulf of Mexico, or carry the pendulum from the sea level to the top of Pike's Peak; the height in the latter case having the same effect as the change in latitude in the former.

In the new work proposed by the Coast and Geodetic Survey all experiments are to be made at a given atmospheric pressure. This will be about a mean value of those actually found in practice. Air will therefore be forced into the chamber at mountain stations, and pumped out at the lower ones. It being decided to swing the pendulums in an enclosed space, the interesting question came up, how close can the pendulum be placed to the walls of the chamber, without influencing the time of oscillation; or in other words, how small can we make the box and still have the pendulum swing just as it would in the open air. Experiments were made with boxes of different sizes and shapes. The result generally stated was that the effect of the sides of the chamber only began to be felt when they were within about one inch of the moving body, and that what is known as "skin friction" is more effective than impact friction. That is to say that proximity of the wall to the side of the pendulum has more influence than nearness in front or back of it. The viscosity of the air is indeed an important factor in the investigation, as it is well known that besides the buoyant effect of the air it adheres to the pendulum and is drawn along with it. This influence has been studied both theoretically and practically by Stokes, Green, Peirce and others, and has furnished some fine examples of mathematical analysis.

The temperature of the pendulum is found by means of another pendulum of exactly the same shape, size and material, except that the knife edge is of hard rubber instead of agate. This auxiliary pendulum is suspended inside the receiver. To it is attached a thermometer, whose bulb is encased in filings of the metal imbedded in the stem of the temperature pendulum. The presence of this auxillary instrument has no appreciable effect on the period of the swinging pendulum, either from air disturbance or from the vibratory movement of the support. This fact was carefully determined by experiment.

In the last cruise of the U. S. Man-of-War Pensacola, an officer of the Coast and Geodetic Survey was sent to determine the force of gravity at some stations in Africa, and on some islands of the North and South Atlantic. The computations have just been completed, and the results are in conformity

with the theories exposed in the body of this article. The stations on continents whether in Africa or America show a defect of several oscillations per day in the movement of the pendulum; whereas those islands that are surrounded by a deep sea and considerably removed from continental masses invariably give an excess of gravity. The Barbados, in the West Indies, has about a normal value, which is just what we expect since it is neither surrounded by a deep sea, nor is it very near the South American Continent. Observations were made at St. Helena and Ascension both at the sea level and on the summit of the mountain. The work shows that the mean density of the whole island in both cases is considerably less than that of the rocks found on the surface, so that the attraction of the mountain must in some way be compensated for by the internal structure or composition of its material.

ART. LIV.—*On the Post-Glacial History of the Hudson River Valley;* by FREDERICK J. H. MERRILL.

FROM the post-Glacial deposits in the Hudson River valley may be derived much information as to the conditions prevailing therein subsequent to the retreat from its vicinity of the continental glacier.

These deposits are of two general types: estuary formations of stratified clay and fine sand deposited in still water, and cross-bedded delta deposits of coarser material. They fringe the river shores in terraces between New York and Albany and indicate a long period of submergence, their present altitude above tide showing that the land has been elevated with respect to sea-level since their formation. Their materials were apparently brought into the estuary by tributary streams which dropped the coarser particles near their mouths, while the finer rock flour was carried on in a state of suspension, and was finally precipitated to form beds of clay.

From Albany westward spreads an alluvial formation which attains at Schenectady an altitude of about 340 feet and extends through the Mohawk Valley in terraces which rise in altitude till they merge in the elevated beach plain of Lake Ontario at Rome about 405 feet above tide level. The origin of these terraces has not yet been determined. According to Mr. G. K. Gilbert the raised beach at Rome is that of a lake dammed by ice in the St. Lawrence Valley and flowing out into the Mohawk Valley, which carries its drainage into the Hudson estuary. According to this view the alluvial plain at Schenec-

tady is the Mohawk delta, and the terraces of the Mohawk valley are stream terraces.

Recently Mr. J. W. Spencer* has advocated the hypothesis that the raised beaches of the Ontario basin were formed at sea-level. In this case the Mohawk valley terraces would be estuary terraces homologous with those of the Hudson valley. An examination of these terraces is necessary to determine the point at issue.

The delta of the Hudson River torrent has not yet been studied by the writer, but it will probably be found in the neighborhood of Sandy Hill. A general description of the estuary deposits of the Champlain Period in this region has been given by Professor W. W. Mather (Geol. 1st Dist. N. Y., pp. 148, 149).

Between Poughkeepsie and New York the following streams have formed extensive delta deposits: Wappinger's Creek near New Hamburg, Fishkill Creek, Quassaic Creek at Newburgh, Moodna River at Cornwall, Indian Creek at Cold Spring, Peek's Kill, Cedar Pond Brook and Minisceong Creek at Haverstraw, Croton River, Pocantico River at Tarrytown, Sawmill River at Yonkers and Tibbit's Brook at Van Courtlandt Park, New York City.

The deposits of Peek's Kill or Annsville Cove, as it is now called, are of considerable interest. These names designate the basin which receives the waters of Annsville Creek, Sprout Brook, and Peekskill Hollow Creek, the last of which carries the drainage of several long and deep valleys trending to the northeast through Putnam County. About the margin of the basin are several terraces about 120 feet high showing characteristic delta structure and on the west bank of the Hudson opposite the village of Peekskill and immediately south of Jones' Point is a terrace of coarse gravel which has the same altitude as those on the east bank and which was, at one time, regarded by the writer as a portion of the Peek's Kill delta deposit. The coarseness of its material, however, would seem to preclude the possibility of this and to suggest that it originated as a moraine or a kame and was subsequently terraced in the waters of the estuary. On the flank of Crow's Nest Mountain near West Point the base of the terrace exposed in the railroad cutting is formed of bowlders of considerable size and it is suggested by Mr. G. K. Gilbert that this deposit was formed by a lobe of the retreating glacier.

The estuary deposits of the Hudson River at New York indicate a post-Glacial depression of more than 70 feet. The terraces which border the west shore of Manhattan Island from

* This Journal, vol. xl, p. 443 *et seq.*

75th street northward have a maximum height of 70 to 75 feet and on the New Jersey shore of the river, terraces of about the same altitude occur at frequent intervals. One of the most prominent of these is at Fort Lee, south of the steamboat landing. The surface material of these terraces is a fine sand or silt easily transported by the wind. It is evidently not a material which could be laid down in running water, for it would be carried in suspension by a river current and could only be precipitated in the still water of an estuary. North of New York City the altitudes of the terraces have been determined at a few points as follows:

Mouth of Croton River	100 feet.
Peekskill	120 "
West Point	180 "
Fishkill	210 "
Schenectady	340 "

A detailed measurement of the terrace altitudes between Fishkill and Schenectady has not yet been made.

On the Long Island Sound shore of Westchester County, N. Y., the till which covers the metamorphic rocks has apparently been levelled off by wave action at an altitude of 75 to 85 feet. Plains of this character occur at frequent intervals, being separated by river valleys, and were probably formed during the depression which occasioned the estuary deposits of the Hudson River valley. These plains are composed of a modified till, obscurely stratified, somewhat sandy near the surface and comparatively free from bowlders, but unaltered bowlder clay or till occurs at a few feet below their surface. On one of the most extensive of them the village of New Rochelle has been built.

On Staten Island and western Long Island alluvial plains of stratified material rise gently from the ocean shore to the margin of the moraine, terminating at an altitude of about 80 feet, and, though no continuous shore-line is to be found, the plains are referred provisionally to the same period as the estuary deposits a few miles north.

From the evidence quoted the amount of the post-glacial depression at New York is estimated at about 80 feet. Whether this was subsequent to a greater depression of post-glacial date remains to be determined.

In the estuary which occupied the Hudson River valley during the depression, there was deposited a great depth of plastic clay, evidently a sediment of aluminous rock flour produced by glacial attrition, and held in suspension by the post-glacial streams, and resting upon this clay, is a deposit of fine stratified sand. This bipartite character of the Hudson River estuary

formation suggests that two distinct conditions prevailed during the time of its deposition. The clay represents a period of still water deposition when little or no siliceous material was washed into the basin, while the overlying stratified sand was evidently deposited at a time when much siliceous matter was carried in by the tributaries.

The causes of this differentiation are not clearly manifest. If there were good evidence to show that, at the close of the ice-period, there had been a greater submergence of the continental margin than that proven by the delta deposits above mentioned, the hypothesis might be advanced that during this greater subsidence there was but little of the land surface exposed above sea-level in the vicinity of this estuary and consequently but little surface drainage. The larger tributaries fed by waters from the melting glacier would then bear into the estuary a large quantity of rock flour which would be held in suspension for a time and would finally be deposited in the deeper water as clay. As the land rose from its submergence, however, a larger area would be exposed to surface drainage and would yield in immediate proximity to the basin, an increasing amount of siliceous matter which would be deposited over the clay and constitute the upper member of the estuary formation.

It remains for future investigation to determine the total amount of submergence in this region cotemporary with the last advance of the ice sheet and subsequent to its retreat.

The records of ocean wave action are in many cases different from those of the extinct Quaternary lakes and not so easy to recognize. It is not always possible to decide a question of submergence by the presence or absence of a distinct shoreline. On a lake shore wave action tends to cut in an horizontal plane and the result is a series of terraces or a beach plane associated with shore drift and littoral deposits in various phases. When ocean waves act upon a shore there may be two cases:

1. The land may be at rest. In this case the result will be the same as on the shore of a lake which maintains its level for a comparatively long time.

2. The land may be rising or subsiding with respect to sea-level. In this case the plane of erosion will be a resultant of two planar forces: *a*, the wave force which operates in an horizontal plane; *b*, the force of elevation or depression which acts in a vertical plane and subjects to the former successively lower or higher portions of the land margin. According to the relation of these forces or the relation of the rate of land movement to the rate of wave cutting the plane of erosion will vary in its inclination. As the cutting rate relatively increases

the plane of erosion approximates to horizontality, and when it becomes infinitely great the plane of erosion will become a base-level. As the cutting rate relatively diminishes, the plane of erosion will become more and more inclined to the base-level and will approach verticality. When the cutting rate becomes infinitely small with respect to the rate of land movement the plane of erosion becomes vertical. In this case a vertical rock face would not lose its verticality by the erosion nor would the slope of the land surface be altered except through variations in the resistance of the rock acted upon.

The degree in which the eroded land surface would approximate to an oblique or vertical plane of erosion would depend upon the previous configuration of that surface. In order to completely discuss this question it would be necessary to consider a large number of incidental factors which might divert the plane of erosion from its theoretic position and prevent the eroded land surface from coinciding with it, but this completeness is unnecessary for the present purpose which is simply to point out the fact that a land surface in process of subsidence or emergence may be subjected to wave action without being incised with distinct shore lines, and also that *wave action may produce an inclined plane as well as a terrace or a base-level.*

It is therefore evident that submergence would not leave a deeply cut shore-line as its record unless the rates of land movement and wave cutting were so adjusted as to permit of it. In fact, no very distinctly cut shore lines are to be found on the drift about New York even at an altitude corresponding with that of the Hudson estuary deposits. Apart from the still water deposits the 80 foot post-Glacial depression about New York can only be traced by change of surface slope and material at this level. Even these two varieties of evidence are not always co-existent.

There are in Westchester County and on Long Island indications of wave action on the glacial drift at altitudes of 150 to 180 feet, it remains to be determined whether they are reliable.

The present condition of the Quaternary deposits in the Hudson valley is indicative of fluviatile erosion in post-Champlain time. The estuary deposits and deltas have been eroded and truncated until but a narrow fringe is left of formations which once extended far across the valley or filled it, and the water in the channel of the river has now a depth varying from 50 feet in the shallower portions to 180 feet in the deepest parts.

The delta deposits have also been subjected to the erosion of the streams which formed them and which developed cutting power as the land rose from its submergence. This erosion removed a large portion of the deposits and excavated chan-

nels through them below present tide-level. The mouths of the tributary streams are now generally silted up and the process of filling seems to be going on at present. It seems indisputable that the brick clay deposits once filled the entire valley up to a certain level, and that the present depth of the channel of the Hudson is due to the erosion of the still water deposits by a river current. It is also probable that in the narrow gorge of the Highlands some of the deltas filled the valley, but this point has not been fully determined.

Between Poughkeepsie and Albany at many points near the water's edge are steep, unglaciated rock surfaces much fresher in appearance than the glaciated surfaces upon which the Champlain deposits rest. These may be the result of river erosion subsequent to the formation of the terraces.

The evidences of fluviatile erosion enumerated suggest a rapid flow of water down the Hudson valley in the late Quaternary. Such a flow doubtless began when the valley rose from its submergence. With these evidences of erosion may be correlated the gorge of the Narrows at the entrance of New York harbor. This is a gap in the terminal moraine about 240 feet deep and one mile wide at tide-level and there is no evidence that it could have resulted from non-deposition of the drift. The bottom of the present channel has a maximum depth of 100 feet below tide-level.

It seems highly improbable that the present navigable channel of the Hudson could have been excavated to its present depth in the Champlain deposits by any agency except that of a river current,* and taking the maximum depth of the channel in the Narrows as an example of this erosion we have the amount of post-Champlain subsidence suggested as about 100 feet in the vicinity of New York.

Observations on the coast of New Jersey and Long Island have well established the fact of recent subsidence which can be measured to the extent of 20 feet, by submerged tree stumps. The evidences of fluviatile erosion in the Hudson valley suggest that this may be not more than one-fifth of the total amount.

From the evidence quoted it may be stated provisionally that after the retreat of the continental glacier from the Hudson River valley, the land stood for a long time at a lower level than at present. What the maximum of depression amounted to is not known but in the vicinity of Albany the minimum depression amounted to about 340 feet and at New York to about 80 feet. Next occurred a gradual elevation of the land amounting to about 180 feet at New York and at Albany to an amount undetermined, but probably not less than

* See J. D. Dana, this Journal, vol. xl, p. 435.

350 feet and perhaps 400 feet or more. During this elevation occurred extensive erosion of the Champlain estuary deposits in the river valley and subsequently followed a depression which has amounted to about 100 feet at New York and which is apparently continuing at the present day.

As the land rose from its 80 foot depression at New York there seems to have been a brief period of less rapid elevation during which a second series of estuary terraces and alluvial plains were formed which now stand about 25 feet above tide-level. These have been recognized on Staten Island by Dr. N. L. Britton and may be seen on the Harlem River near Fordham Heights and at various points on the Long Island Sound shore of Westchester County.

ART. LV.—*On Alunite and Diaspore from the Rosita Hills, Colorado;* by WHITMAN CROSS.

THE occurrences to be described in this article lie between the mining towns of Silver Cliff and Rosita, in Custer County, Colorado. They were discovered while studying the geology of this region, under the direction of Mr. S. F. Emmons, preliminary to a report which will appear as a monograph of the U. S. Geological Survey. In order that the geological interest attaching to the occurrences may be fully understood a general sketch of the local geology will be given.

I. *Geological Sketch of the Rosita Hills.*

The name Rosita Hills has been applied in the course of this work to a small group of rounded hills on the eastern slope of the great Wet Mountain Valley, which lies between the Sangre de Cristo and Wet Mountain [or Greenhorn] ranges, at a point south of the Grand Cañon of the Arkansas River. They cover an area whose dimensions are about five miles north and south, by four east and west, in which are small cones and smooth-sloped ridges, whose absolute elevations vary from 8,900 to 9,700 feet above sea-level, while the western and lower base of the hills is at 8,500 feet. The Rosita Hills are made up of volcanic rocks, while Archæan schists surround them on all sides and constitute the floor upon which they rest, there being no sedimentary formations in the vicinity, excepting local tufa beds. Upon the Hayden Geological map of Colorado, Dr. F. M. Endlich being responsible for this portion, the Rosita Hills are included in a much larger area of eruptive rock ["trachoreite," Endlich.], represented as extending along the base of the Wet Mountains. In this connection it may

not be out of place to state, for the information of those who have no grounds upon which to establish a personal opinion, that the term "trachoreite" of Endlich has no petrographical signification whatever. Almost all varieties of volcanic rocks known in Colorado—a long series—may be found prominently developed in areas mapped as "trachoreite" by Dr. Endlich.

The Rosita Hills are remarkable, when compared with other volcanic areas of the West, for the number of eruptions and the variety of products in so limited a district. Volcanic activity began, as indicated by the products seen, with an eruption of an andesite carrying hornblende and biotite. The action was explosive, for the product is wholly fragmental, consisting of mud, tufa, and breccia, now exposed in very irregular relations. The vent is not known, and probably lies under some later flow. After erosion of the soft materials of the first period came two massive andesite outbreaks, one more basic, the other more acid, than the first. These overlap the earlier breccia on the north and south respectively, and form prominent cones and ridges.

Succeeding these andesitic eruptions came a series of rhyolitic outbursts. The earlier ones were violently explosive as shown by the agglomerate filling some of the vents, while the later ones were more quiet, producing massive rocks, seen in many short dikes cutting all the earlier andesites and the rhyolitic agglomerate, and in thin sheets on all flanks of the hills. Following the rhyolite came another andesitic magma, welling out through long fissures which cut all earlier rocks. Surface masses of the same rock are seen. It is a mica-augite andesite, with some free silica. The last important eruption produced a rock carrying a very slight excess of silica and having the characteristic structure and mineral composition of a trachyte. This magma came up through fissures some of which are nearly three miles long, and clearly traverse every rock that has been mentioned, excepting the dacite, which does not lie in their course. The later rocks of this series are in many places very fresh, while the older andesites are as a rule far gone in decomposition. This general decay is mainly due to thermal waters coursing through innumerable fissures. In these decomposed areas are many small, metal-bearing mineral veins.

The area whose volcanic history has thus been outlined is regarded by the writer as practically a volcano, whose phases were of very different character at different times. Four out of six important outbreaks produced massive rocks and but two were of the explosive character more commonly seen in true volcanoes. But the integral nature of the whole is evident from the study of the mutual relationships of the

rock masses. As a further proof that typical volcanic action has occurred here stand the masses of decomposed rhyolite which are to be described, for they can only be explained on the supposition that the rhyolitic outburst, known to have been of violently explosive character, was followed by a period of sulphurous gaseous exhalations whose products are identical with those of well known volcanic regions. There were two vents in particular which thus became true solfataras, and about them the rhyolite has suffered change into a rock-mass affording unusual resistance to atmospheric agencies and now forming rugged cliffs and projecting outcrops in a region of prevailingly gentle slopes and rounded contours. These two localities are in Democrat Hill and Mount Robinson, both situated in the inner part of the group of the Rosita Hills.

II. *The alunite rock of Democrat Hill.*

Democrat Hill is situated in the center of the Rosita Hills and in the acute angle between two gulches. It is of general rounded shape at the base, with a diameter of 1,500 feet and rises only 400 feet. On the north, or opposite the forks of the gulch, it joins on to a long ridge of andesite, and indeed all to the north is andesite, excepting later dike rocks, while to the south the prevalent rock is rhyolite, chiefly in the form of flows, some of which issued from the conduit below the hill.

The upper three hundred feet of Democrat Hill stands out as a rough massive knob whose projections are somewhat rounded, though split by fissures and presenting occasional cliff faces. The color is slightly reddish and the whole presents a strong resemblance to certain outcrops of massive granite. The lower hundred feet of the hill are covered by great angular blocks which have fallen from the cliffs above. A close examination of the rocks shows it to be cellular, the cavities being of irregular shape and varying in size, with a maximum diameter of several inches, and an average of about one inch. The cells are lined by rudely tabular crystals some of which are composite and all are obscured by the minute quartz crystals deposited upon them. The mass of the rock is an irregular aggregate of imperfectly tabular grains of a mineral closely resembling orthoclase in luster, hardness, and general appearance. A pronounced cleavage runs parallel to the dominant planes of the tablets. The only other constituent of the rock is quartz, which forms a very evenly and finely granular mass between the tablets, and its grains are also abundantly included in the latter. A small amount of snow-white kaolinite is sometimes seen in the cavities.

Microscopical examination of thin sections of the rock, and of cleavage flakes, shows the questionable mineral to be uniaxial,

positive, and the cleavage is parallel to the basal plane. Sections at right angles to the cleavage show stronger polarization than in feldspar, and sharp extinction parallel to the cleavage lines. The rough crystals in the cavities have the same optical characters, and the more perfect of them have a hexagonal outline caused by apparently rhombohedral planes. A chemical analysis of the average rock was made by Mr. L. G. Eakins, in the laboratory of the U. S. Geological Survey, with the following results:

			Molec. ratio.		
SiO_2	65·94				
Al_2O_3	12·95	÷ 102	·127		3·26
K_2O	2·32	÷ 94	·025	·044	1·13
Na_2O	1·19	÷ 62	·019		
SO_3	12·47	÷ 80	·156		4·
H_2O	4·47	÷ 18	·248		6·36
Fe_2O_3 etc.	0·55				
	99·89				

From the above figures it is seen that the constituents of alunite are present in very nearly the molecular proportion required for that mineral, with a slight excess of water and of bases. It is quite probable that there was a small amount of kaolinite in the material analyzed. The sulphate present is slowly soluble in H_2SO_4, and after slow roasting alum can be extracted with water. This rock then is made up of quartz, two-thirds, and alunite, one-third, aside from insignificant impurities. The specimen analyzed seems to be representative of the entire upper part of the hill, though the percentages of quartz and alunite doubtless vary somewhat. Rhyolite occurs on the lower slopes about it and also underneath the summit of the hill, as shown by tunnels. A transition from rhyolite to alunite rock has not been observed at this place, but no good reason is known for doubting that rhyolite was the original rock here as at the other locality to be described. The observed limitation of the alunite rock in depth no doubt corresponds with the horizon at which sulphureted hydrogen gave rise to sulphurous acid on oxidation near the surface.

III. *The alunite-diaspore rock of Mt. Robinson.*

Mt. Robinson is the highest point of the Rosita hills, although the summit is but six hundred feet above its southern base. Its slopes are smooth, owing to the soft andesitic material beneath, but the top is a projecting ridge, a quarter of a mile long, with a cliff of from fifty to seventy-five feet in height on the south, while a growth of aspens come close up to the crest on the north. The jagged crest is fifty feet or more in width,

and is made of a hard, rough, porous rock—a decomposition product, varying locally in character, but exhibiting in few places any trace of the original rock structure. The crest represents the extreme alteration. Along the base of the summit cliffs is a plain contact between spherulitic rhyolite and andesite, and at either end of the crest the harder rock gives way to modifications showing spherulitic structure, and in one direction the body is continued for some distance as a distinct rhyolite dike, cutting through andesite.

The rock composing the rough outcrops of the summit ridge is often much like the alunite-bearing mass of Democrat hill, and is in places identical with it, but it is much less uniform in character. Certain masses are composed of bluish cellular quartz, and barite appears developed in great irregular tablets in a few spots. On the whole, alunite is not developed in so large grains as in Democrat hill, and through the numerous minute quartz grains included in it the cleavage faces are less distinct, the result being a dull whitish or slightly yellowish rock of dense texture save for small irregular pores containing kaolin or yellowish ochre. A specimen of such rock from the eastern end of the crest, whose composition cannot be made out macroscopically, but which exhibits a large amount of alunite in thin section, was analyzed by Mr. Eakins and found to contain :

		Molec. ratio.	
SiO_2	69·67		
Al_2O_3	13·72	·134	4·8
CaO	0·07		
MgO	*tr.*		
K_2O	2·44	·032	1·1
Na_2O	0·34		
SO_3	9·27	·114	4·0
H_2O	4·73	·263	9·4
	100·24		

The alkalies present are but very slightly in excess of the amount required to go with the sulphuric acid to form alunite, while there is a considerable residue of alumina and water belonging to kaolin, the presence of which is shown by the microscope. The percentage of SO_3 found corresponds to 23·96 per cent of alunite.

Toward the west end of the crest-ridge, and also near the middle, there is much of a rough, finely cellular rock consisting almost exclusively of bluish quartz and a transparent colorless mineral in irregular grains, noticeable on account of the bright luster on a very perfect cleavage plane. This mineral was at first supposed to be alunite in an unusually pure state. With

this idea in mind, a specimen of the rock taken at the west end of the summit ridge was analyzed by Mr. Eakins, with this result:

SiO_2	76·22
TiO_2	0·11
Al_2O_3	19·45
Fe_2O_3	*tr.*
CaO	*tr.*
Alk.	*tr.*
SO_3	0·29
P_2O_5	0·13
H_2O	3·82
	100·02

The alumina belonged to a mineral insoluble in most acids, and infusible in alkaline carbonates. On treatment by hydrofluoric acid, which dissolves the mineral with great difficulty, 17·79 per cent of this substance was isolated from the rock, and found to contain 84·67 per cent of Al_2O_3, with no other base, while water was present in large amount. The theoretical composition of diaspore is: Al_2O_3 85·07, H_2O 14·93 = 100. Microscopical examination of the rock in thin sections, and of cleavage flakes of the mineral in question, showed it to possess the physical and optical properties of diaspore. By reason of its high refractive index its surface relief in thin sections distinguishes it clearly from alunite, although both minerals occur in irregular grains in the rock mass and are filled by small included quartz grains.

The alunite and diaspore of this rock having been determined in the spring of 1890, the writer revisited the region in the following summer, obtained further information concerning the occurrence, and collected specimens of particular interest. On carefully examining the material from a prospect hole sunk in the quartz-diaspore rock analyzed, some specimens were found containing irregular cavities an inch or more in diameter, in which were groups of rather stout prismatic, colorless or whitish crystals, with glistening faces, though seldom transparent. The crystals have several planes in the prismatic zone, and the low terminal planes which are clearly pyramids and domes indicate the symmetry of the orthorhombic system. Much of the surface rock at this end of the dike has cavities with similar crystals which are dull white in color and clearly much decomposed, the product being a fine micaceous mineral apparently kaolin.

On the southern slope of the dike, below the summit, a few loose fragments of quartzose rock were found, with very brilliant, clear, prismatic crystals, inclined to tabular development

through the prominence of a pinacoid. While these various forms were thought to belong to a single mineral, the species was not recognized in the field. By measurements, on a small Fuess goniometer, the mineral was, however, readily identified crystallographically with diaspore, and the faces determined as follows: $\infty P \breve{\infty}$, prominent in all crystals; $\infty P\bar{2}$, the most prominent prism; ∞P, narrow; $P\bar{2}$, broad, good faces; unit pyramid, P, narrow; $P \breve{\infty}$, occasionally distinct. As this seems to be the first known occurrence of diaspore in any such connections, and also on account of the rarity of this crystallographic development, the writer requested Dr. W. H. Melville to examine the material carefully, and if the result warranted it, to present the crystallographic data, with figures, in a special article. This he kindly consented to do, and his report will be found in the paper succeeding this. A small clump of clear crystals was analyzed by Mr. Eakins with this result:

Al_2O_3	83·97
H_2O	15·43
	99·40

The results of the crystallographical and chemical examinations thus place the identity of the mineral with diaspore beyond question. Further data as to the occurrence will be given in discussing the origin of these minerals.

IV. *Alunite pseudomorphs.*

At the western base of the Rosita hills a shallow prospect shaft has been sunk in a brecciated quartzose vein matter, which is the alteration product of the country andesite on a line of fissuring. Near by is a large dike of altered rhyolite, the specimens from it which have been examined consisting of kaolin and quartz.

The shaft has long been abandoned and is inaccessible. Its dump shows mainly quartz vein matter through which some pyritiferous ore is sprinkled. One small part of the dump is made of bluish quartz breccia, the spaces between angular fragments being lined by crystals. There is first a coating of minute quartz prisms, then in some cavities larger quartzes with rough prismatic and smooth pyramidal faces. In a portion of this material there are numerous tabular crystals, and, of decidedly later age, kaolin, or ochreous limonite, in a few specimens.

The tabular crystals referred to are dull white, opaque, with rough surfaces, yet showing distinct crystal form. The faces are to be interpreted as unequally developed positive and negative rhombohedrons, combined with a dominant basal plane. In size these crystals average 0·5^{cm} in width, by a thickness of

$\frac{1}{2}^{mm}$. An examination with a hand lens shows that the crystals are for the most part irregular granular aggregates of some mineral possessing a distinct cleavage. This is especially clear on fractured surfaces by the irregular positions of the cleavage planes. In some of the thicker crystals, broken through the center, is seen a clear glassy kernel of a colorless mineral, and its position immediately suggests that it represents the original substance of the crystal, of which the granular aggregate about it is a pseudomorphic alteration product.

Some of these white crystals, carefully detached, but supposed to include the points of some of the quartz crystals upon which they were deposited, were given to Mr. Eakins for analysis. He found:

		Molec. ratio.	
Al_2O_3	38·91	·381	3·42
K_2O	4·03	·113	1·01
Na_2O	4·32		
SO_3	35·91	·446	4·00
H_2O	13·03	·724	6·49
CaO	0·35		
SiO_2	2·82		
MgO	*tr.*		
	99·37		

Tests showed that the silica came chiefly from included quartz grains. The remaining constituents, aside from the small amount of lime, are those of alunite, and by the usual calculation it is seen that they are present in the required ratio for that mineral, with a slight excess of water and of alumina, which can be referred, with a portion of the silica, to admixed kaolin, an observed associate of the crystals.

The unexpected result of the analysis was followed by a microscopical examination of thin sections prepared parallel to the basal plane of several crystals, and normal to that plane in a crystal with a glassy kernel. The fresh core possesses a cleavage parallel to the basal plane of the crystal, and its optical properties seem to be throughout those of alunite. The outline of the kernel is irregular. About the fresh alunite kernel is an aggregate of irregular particles, arranged without reference either to the outer crystal form or to the inner core. Those grains which are decidedly elongated polarize brightly, have sharp lines of cleavage parallel to the length, and the direction of major elasticity is always parallel to the cleavage lines. Many sections are not so elongated, and these polarize less brightly, some giving only gray tones, while a few are almost isotropic. The latter show a positive interference cross in convergent polarized light.

The facts which have been given seem to prove that the crystals in question are pseudomorphs of alunite after itself. An explanation of this anomaly is suggested by the volcanic history of the district. The crystals are almost certainly the result of solfataric action connected with the occurrences already described. If we suppose those alunite crystals to have been replaced by some other mineral, in which the bases were retained, a renewal of solfataric activity would naturally convert them into alunite again, but the protected cores of the original mineral could not influence the orientation of the new generation.

V. *Origin of the rocks described.*

In all of its observed occurrences alunite is a secondary product due to the action of sulphurous or similar acids upon highly aluminous rocks, yet there are two very different sites, with different attendant conditions, in which this action takes place. In the one case, the rock belongs to a clay bank and the acid is derived as a rule from the decomposition of marcasite in or adjacent to the clay. The product is a dark dense amorphous mass. In the other case, the agent is the sulphurous exhalation of a solfatara, and the rock acted upon is usually volcanic and rich in alumina and alkali—such a rock as rhyolite or trachyte. The product of this action is commonly a hard, rough, porous, highly crystalline rock, though a dense amorphous mass is often locally developed.

The alunite rock which has been described is directly comparable with the classic solfataric occurrences of Bereghszasz in Hungary, the island of Milo, and La Tolfa, near Rome. The writer has been unable to find mention of any similar occurrence on the American continent, and, judging from the descriptions given by von Richthofen* and vom Rath† of other localities, the rock of Democrat hill is remarkable for its purity, homogeneity and extent. Exactly similar material is not described by the authors named.

The limits of this article forbid any further comparison of occurrences or discussion of the processes involved in the formation of alunite, which are reserved for the monographic report upon the geology of the district.

While the alunite may be assumed to have been formed by the same processes which are involved in other cases, the diaspore of Mt. Robinson has apparently originated under conditions very different from those of any other known occurrence.

* In "Studien aus den ungarisch-siebenbürgischen Trachytgebirgen" Jahrbuch d. k. k. geol. Reichsanstalt, xi, 254–268, Vienna, 1860.

† Mineralogisch-geognostische Fragmente aus Italien. IV. Das Bergland von Tolfa. Zeitschrift der deutschen geol. Ges., xviii, 585, Berlin, 1866.

Its genesis, when in the ordinary association with corundum, emery, margarite, and other minerals, can have little in common with the present case. It has never been reported as an associate of alunite, nor as a decomposition product of eruptive rocks. The only previously observed occurrence connecting diaspore in any way with eruptive materials was recently mentioned by A. Lacroix,* who identified it as a minor constituent of a single block of gneiss, enclosed in a basaltic tufa of the Auvergne. This block is composed mainly of garnet, quartz, orthoclase and oligoclase, with rutile and diaspore, and is considered by Lacroix as an ejected fragment of a much metamorphosed rock, like many others in association with it. He does not refer to the novelty of this occurrence for diaspore, nor discuss its origin.

There is but little evidence upon which to formulate a theory as to the origin of the diaspore of Mt. Robinson, but after study of the specimens, it seems to the writer probable that it is here a result of the destruction of alunite At La Tolfa, alum is obtained by the slow roasting of the alunite rock, after which water extracts the soluble sulphate, but there is a residue of insoluble hydrate of alumina. Should this roasting and leaching take place in nature, it seems quite likely that subsequent conditions might lead to the crystallization of the residue as diaspore, making a porous quartz-diaspore rock. Some further agency would seem to be required to explain the occurrence of diaspore crystals in the cavities.

Art. LVI.—*Diaspore Crystals;* by W. H. Melville.

The crystals of diaspore, which Mr. Cross submitted to me for examination, present two types of combinations of planes referable to the prismatic or orthorhombic system of axes. One type consists of light brown transparent crystals which are elongated in that direction commonly chosen as the vertical axis, and which exhibit a largely developed brachypinacoid plane invariably much striated. This latter characteristic is true though to a less extent for the narrow prismatic planes. The crystals are doubly terminated, and are implanted in the associated rock upon one set of prismatic edges. A drawing of these crystals would resemble that given in Dana's Mineralogy, fifth edition, page 168, fig. 173. The following forms were observed.†

* "Sur l'existence d'une roche à diaspore dans la Haute-Loire." Bul. Soc. fran. de Min., xiii, p. 7, Jan., 1890.

† The symbols given are those of Miller and of Naumann as modified by Dana.

Brachypinacoid.		Brachydome.
[100], i-$\bar{1}$	[120], i-$\bar{2}$	[101], 1-$\bar{1}$
Prisms	[340], i-$\frac{4}{3}$	Pyramids
		[111], 1
[110], I	[670], i-$\frac{6}{7}$	[122], 1-$\bar{2}$

In the zone circle [111, 122] the mean of the most accurate measurements are given* under I, and another series under II. In almost all cases the signals reflected from the brachypinacoid were duplicated, so that it was often necessary to calculate the angle which this plane made with its adjacent planes from the difference of the sum of the other angles in the zone and 180°.

	Measured. I.	II.	*Kokscharof.
100 ∧ 111	63° 7	63° 20½	63° 5½
111 ∧ 122	12° 41½	12° 44	12° 40
122 ∧ $\bar{1}$22	28° 23	28° 23	28° 29
$\bar{1}$22 ∧ $\bar{1}$11	12° 41½	12° 39	12° 40
$\bar{1}$11 ∧ $\bar{1}$00	63° 7	63° 21½	68° 5½
	180°	180° 28′	

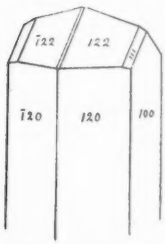
In the zone [100, 101] the measured angles were 100 ∧ 101 = 58° 50′ (1) and 58° 54′ (2); mean 58° 52′. For this angle Dana gives 58° 52½′ and Marignac 58° 53′. The following are the mean of the most reliable angles obtained in the zone of prisms:

100 ∧ 110	46° 54½	110 ∧ 670	4° 22	670 ∧ 340	3° 7½
340 ∧ 120	10° 20½	120 ∧ $\bar{1}$20	50° 31		

The axial ratio was calculated from the angles 100 ∧ 110 = 46° 54½ and 100 ∧ 101 = 58° 52.

$$\dot{a} : \breve{b} : \dot{c} = 0{\cdot}6457 : 1 : 1{\cdot}0689 \text{ found.}$$
$$= 0{\cdot}64425 : 1 : 1{\cdot}067 \text{ Dana.}$$

The second type consists of crystals which are white and almost opaque. They are stout and often present a pyramidal habit, as they are imbedded in the rock, resulting from the combination of the prism (120) and the octahedron (122). The annexed figure illustrates this habit of diaspore, and is drawn from a crystal which was removed from the rock and measured. The dominant forms are (120) and (122), while a macrodome (011), the brachypinacoid (100), and the octahedron (111) appear as small secondary planes. The crystals rarely show more than can be seen in the drawing, because they have grown into each other forming dense aggregates. The following angles were obtained from this crystal:



* The angles between normals are given in all cases.

	Between normals.			Between normals.	
$100 \wedge 111$	64° 5	approximate.	$122 \wedge 120$	54° 26	excellent.
$111 \wedge 122$	12° 3	"	$\bar{1}22 \wedge \bar{1}20$	54° 17	"
$122 \wedge \bar{1}22$	28° 20	excellent.	$120 \wedge \bar{1}20$	50° 7	

It is apparent from the foregoing tables that the crystals under discussion do not differ essentially in crystalline habit from those of the same species previously described by other mineralogists. The slight differences in the recorded measurements must be due to the imperfection of the surfaces from which the light is reflected. Reflected images of the slit of the goniometer have been frequently observed superimposed upon each other, and differing in position by two and three minutes. Errors arising from this source have been eliminated to a great extent in my tables of measurements by the choice of the mean value of many observations.

Chemical Laboratory of the U. S. Geological Survey,
Washington, D. C., Feb. 24th, 1891.

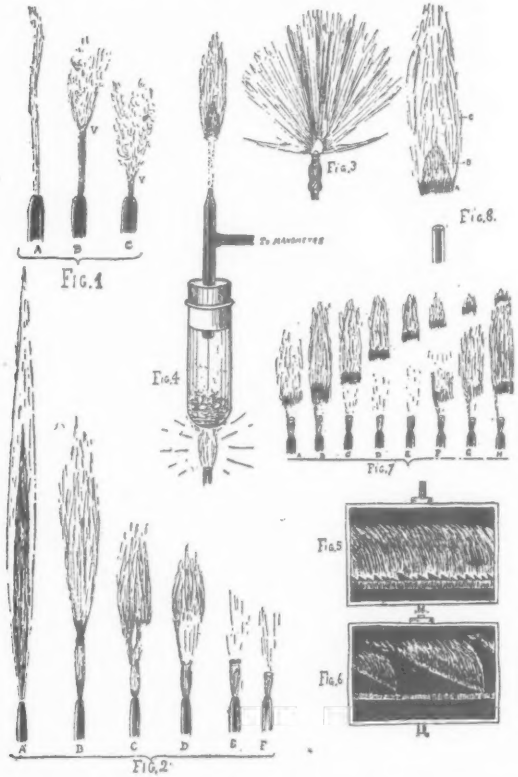
Art. LVII.—*Combustion of Gas Jets under Pressure;* by R. W. Wood, Jr.

Any one who has watched a burning jet of ether vapor has probably noticed that, as the pressure increases, the flame gradually retreats from the orifice, hovering in mid-air, as it were, and eventually goes out, if the pressure be carried beyond a certain point. In endeavoring to discover the exact cause of this, a number of rather curious phenomena were observed. Different gases were experimented upon, and found to act in very different ways, dependent apparently upon the relative amounts of oxygen required for their combustion.

The first experiments were made with coal gas, with pressures varying from 0·5 to 25^{cms} of mercury. The gas was drawn from a large reservoir, used for supplying an oxy-hydrogen lantern, at a tension of about ten pounds to the inch, the pressure at the jet being regulated by a stop-cock and measured by an open U-form mercurial manometer. With an orifice 1^{mm} in diameter, the following were the results obtained under varying pressures.

Pressure 0·5^{cms}—A quiet cylindrical flame, like a candle, 17^{cms} high (fig. 2 A). *Pressure* 1^{cm}—Flame 26^{cms} high. *Pressure* 1·4^{cm}—Flame 33^{cms} high, still quiet and tapering to a fine point; this pressure gave the maximum illuminating power to the flame. On increasing the pressure, the flame suddenly shortened, diminished in luminosity, and vibrated considerably, emitting a slight roaring sound which increased with the pres-

sure. *Pressure* 2^{cms}—Flame reduced to a height of 20^{cms}; hissed perceptibly. *Pressure* 3^{cms}—Its length about 17^{cms}; it roared loudly, and a contracted place appeared 4^{cms} from the orifice, fig. 2 B). *Pressure* 5^{cms}—Flame reduced to 15^{cms}, the contraction approached within 1·7^{cms} of the orifice. Color, blue for the most part, a little yellow at the tip, very little luminosity.



Pressure 7^{cms}—Yellow tip all gone; small broken spaces appeared in the flame beyond the contraction; resembled the flame of a Bunsen blast lamp and was about as hot; glass could be worked in it as easily as in the blowpipe flame (fig. 2 C). *Pressure* 10^{cms}—A large break appeared in the flame just beyond the contraction; the flame beyond united with that below by a mere thread (fig. 2 D). *Pressure* 11^{cms}—Flame beyond the break alternately went out and relighted with a snapping noise, showing that it was an explosive mixture of gas and air.

Pressure 12^{cms}—Flame beyond the contraction went out, and there was left only a short tube of blue fire, as sharply defined as if made of rigid matter (fig. 2 E). Through this tube a vast quantity of unconsumed gas flowed, which was so cold that the eye could be brought directly over the tube, when it was seen to be distinctly hollow, with thin sharply defined walls. *Pressure* 20^{cms}—Tube shortened to one-half its former length, and the walls became thinner (fig. 2 F). *Pressure* 23^{cms}—The tube went out.

The formation of the tube is a rather curious phenomenon. It appears to be due to the fact that the gas molecules on the outside are moving slower than those within, the speed being reduced by friction with the walls of the orifice. What we have then is a jet of gas moving at high speed surrounded, below the contracted place (where the air mixes with it), by a shell of gas moving at a velocity so low that it will remain ignited; with an increase of pressure we should expect the shell to become thinner, and this is exactly what happens. Burning the gas in oxygen, the jet will stand a far greater pressure without forming a tube or blowing itself out. In fact it is difficult to raise the pressure high enough to bring this about. Were we dealing with a flame of pure hydrogen, burning in an atmosphere of oxygen, it would be impossible. Any one who has used the oxy-hydrogen blowpipe knows that it is impossible to give a velocity to the mixed gases great enough to extinguish the jet. The rate at which flame will run down such a jet, or rather the rate at which combustion takes place through a mixture of two parts of hydrogen and one of oxygen is 2500 meters (more than a mile and a half) a second. That is to say, if we had a tube a mile and a half long filled with the mixture, the flame would traverse it in one second. The explosion would be practically instantaneous. Contrast this with coal gas and air. Fill a glass tube a meter long and an inch in diameter with a mixture consisting of 1 part of coal gas to 10 of air. Apply a flame to the open end, and a disc of blue fire will descend the tube with a whistling noise at the rate of about 18 inches a second! The reason why the jet of coal gas blows itself out is very obvious; its velocity is greater than 18 inches per second, consequently it carries the flame away, so to speak. To cause an oxy-hydrogen jet to act in a similar manner we should have to give it a velocity greater than 2500 meters a second. To determine the exact form of the jet when not ignited, the gas was passed through two bulbs, each filled with asbestus, that in one bulb soaked with ammonia, in the other with hydrochloric acid. The gas, passing through these bulbs, became charged with dense white fumes of ammonic chloride, which rendered the jet plainly

visible. At a very low pressure, too small to be accurately measured by the manometer (say about 0·1^{cm}), the column of gas rose unbroken like the smoke of an extinguished candle (fig. 1 A), and a burning match applied to the top of the column would ignite the jet, the flame running down to the orifice and burning quietly there. On blowing out the flame and increasing the pressure a little, the column broke into a fan at a definite height (V, fig. 1 B). Below this point, the regular outline was preserved, but above it the gas diverged rapidly in whirling masses. To ignite the jet permanently, it was now necessary to apply flame below the point V. If applied above, the gas beyond the match burnt with a roaring blue flame, which went out the instant the match was withdrawn, or if the velocity was not too great, hung for a few moments in mid-air. The point V, where the divergence commences, is evidently the point where air is being mixed with the gas. Increasing the pressure brings the point nearer and nearer the orifice, until finally there is scarcely a trace of the unbroken column.

Let us now consider why it is necessary to bring the flame below V in order to ignite the jet permanently. Above this point there is a mixture of gas and air, which is moving at a rate considerably greater than 18 inches per second, consequently the flame is carried up by the jet faster than it can run down. Below the point V we have a stream of undiluted gas, down which the flame will travel rapidly enough to keep the jet permanently lighted, for then the gas above V will be kept lighted by the flame below. What we should expect would be a quiet cylindrical yellow flame below V, and a roaring, flaring, blue flame above, but this is not what happens. On bringing the match below the point of divergence, the burning mass above ceases to flare, and the whole subsides into a quiet tapering yellow flame. The pressure may be increased some centimeters before the tip begins to flare and burn with a blue flame. The reason for this is not very obvious. Igniting the jet appears to bind it together and prevent entrance of air.

With an ordinary four-foot bat-wing burner the best flame was given by a pressure of about 1·3^{cms} of mercury. A pressure of 2^{cms} gave a quiet flame, but there were lateral horns at the bottom. At 6^{cms} the luminosity was greatly diminished, wide horns formed, and radiating streaks appeared in the blue part. At 12^{cms} the luminosity was nearly gone, and a dark arch of unconsumed gas appeared above the orifice, fig. 3. A pressure of 20^{cms} widened this arch, and rendered the flame non-luminous. At 25^{cms} the flame blew itself out.

These experiments were repeated with ether vapor, the pressure being derived by boiling the fluid. Owing to the

rapid condensation, considerable difficulty was experienced in keeping the pressure uniform during the observations. The tubes leading from the flask to the manometer had to be kept hot in order to prevent fluid ether instead of vapor from being delivered at the orifice, which in this case was smaller, being about the size of a small sewing needle.

With coal gas this jet would stand a pressure of 20^{cms} without going out. With ether vapor $\frac{1}{5}$ of a centimeter gave a flame like a candle, while $\frac{1}{2}^{cm}$ caused the flame to retreat about a centimeter from the orifice and burn in mid-air. At $\frac{7}{10}^{cm}$ pressure the distance had widened to four centimeters, and the vaporized ether could be seen rising as a cylinder the size of a knitting needle surmounted by a hollow blue flame, fig. 4. At 1^{cm} pressure the flame went out! It behaved very much as coal gas did when burnt in bad air, the flame showing no tendency to form a tube under increased pressure. This seems to be due to the fact that ether vapor requires a greater amount of oxygen for its combustion. In pure oxygen it behaves very much as coal gas does in air. Alcohol vapor comes between the two, the flame standing a pressure of about 3^{cms} without being extinguished, and showing a tendency to form a tube.

The nature of these flames was studied with a revolving mirror, but no interesting features were revealed except in the case of the coal gas under high pressure. The action of the rotating mirror is to spread out the flame in a broad band, giving as it were an infinite number of instantaneous views, placed side by side. If the upper parts of the jet move slower than the lower, they will suffer lateral displacement, and the flame images will be curved backwards. The flame shown in fig. 2 C when examined in the mirror appeared as in fig. 5. The serrated part of the flame represents the contracted portion, and the alternate dark and light spaces show that this part of the flame alternately ignites and goes out, with sufficient speed, however, to keep the gas above in a state of continuous combustion. The long narrow band below is the burning tube of gas, and the bright teeth are rapid flashes of fire uniting this with the burning mass above. Under greater pressure the frequency of these flashes becomes reduced to such a degree that they are appreciable to the eye. This is the case in flame D (fig. 1). Here the upper portion alternately ignites and goes out with a rapid snapping noise, the pulses varying from one to ten a second. In the rotating mirror this flame presents the appearance shown in fig. 6, the flashes being so slow that they are completely isolated from each other by the mirror, each one being seen as a broad flare of light. The line representing the tube of fire is narrower now, the pressure being greater. The slanting base of the "flare" is bright blue, above this is a

faint green tinge, while still higher we find the light purple tint of the Bunsen flame. The appearance of this flame in the mirror clearly indicates what is taking place. The mass of gas above the contracted place, after being ignited by a flash from the continuously burning tube, does not immediately go out. The flame fights its way down the ascending column, but continually loses ground, owing to the great velocity of the jet. The second flash comes often before the tip of the first one has gone out, and if the interval is short enough the flashes will mingle. In fig. 7, I have attempted to show this in a clearer manner. The series shows an interval of one flash, the figures representing instantaneous views of the flame at successive moments of time. Figs. A to E show the gradual dying out of the flame of the first flash. At F the second flash has commenced, and at H it has risen and mingled with the last vestige of the first.

By carefully regulating the pressure, we may give to the jet a velocity which shall equal the speed of combustion for the mixture of gas and air. The flame will now hang balanced in the air, as shown in fig. 8, the tendency of the fire to spread downward being exactly neutralized by the upward motion of the column. On examining this flame the cause of the bright blue and faint green lines in the mirror is discerned. Around the base of the flame the mixture burns with an intense blue light (fig. 8 A); surmounting this is a cone of greenish fire B, while above this the flame has a light purple tinge.

ART. LVIII.—*Allotropic Silver. Part III. Blue Silver, soluble and insoluble Forms;* by M. CAREY LEA.

WHEN my first paper on the subject of allotropic silver was published about two years ago, that substance seemed to be the result of a very limited number of reactions closely allied to each other. Further study has shown that it is a much more common product than at first appeared to be the case. Wherever in the reduction of silver a reddish color shows itself, that may be taken as a probable indication that allotropic silver has been formed, even although it may be destroyed before it can be isolated.

What is rather remarkable is that allotropic silver is produced abundantly in certain very familiar reactions in which its presence has never been suspected: so abundantly in fact that some of these reactions constitute the best methods of obtaining silver in the soluble form. In photographic operations silver has often been reduced by tannin in the presence

of alkalies. It has not been imagined that by slightly varying the conditions, the whole of the silver may be made to pass into solution as a soluble metal with its characteristic intense blood red color.

Some of these new reactions will be here described in detail.

Allotropic Silver obtained with Dextrine and Alkaline Hydroxide.

When dextrine is dissolved in a solution of potassium or sodium hydroxide and silver nitrate is added, keeping the hydroxide in moderate excess, the silver is at first thrown down in the form of the well known brown oxide. This brown color presently changes to a reddish chocolate shade and at the same time the silver begins to dissolve. In a few minutes the whole has dissolved to a deep red color, so intense as to be almost black. A few drops poured into water give it a splendid red color of perfect transparency. Examination with the spectroscope leaves no doubt that we have to do with a true solution.

It is interesting to observe that silver can be held in solution in neutral, acid and alkaline liquids. In the first process which I published, in which silver citrate is reduced by a mixture of sodic citrate and ferrous sulphate, the latter may be used either in acid solution or it may be first neutralized with alkaline hydroxide, so that that form of silver is held in solution in either a neutral or an acid liquid. The form that is obtained with the aid of dextrine dissolves most freely in the strongly alkaline liquid in which it is produced, and when dilute nitric or sulphuric acid is added the silver is precipitated. But with acetic acid the precipitation is very incomplete: the solution retains a brown color and contains silver. Even the addition of a large excess of strong acetic acid fails to throw down any more silver. It follows therefore that while this form of silver is most freely soluble in a strongly alkaline liquid it is also soluble to some extent in one that is either neutral or acid.

The precipitate when once formed appears to be almost insoluble. A small portion of it stirred up with distilled water gives no indication of solution. But if a quantity is thrown on a filter and washed, as soon as the mother water is washed out the liquid runs though of a muddy red, and if this filtrate be allowed to stand it deposits an insoluble portion and then has a fine rose-red color and perfect transparency. Notwithstanding the beautiful color it contains a trace of silver only, so great is the coloring power of the metal. Sometimes if the alkaline solution stands for a month or two the silver becomes spontaneously insoluble; most of it falls to the bottom as a deep red substance, but part remains in suspension

with a bright brick red color. The difference between this and the true solution as originally formed is extremely well marked.

Dextrine is a very variable substance and different specimens act very differently. Common brown dextrine seems to do better than the purified forms.

Convenient proportions are as follows: in two liters of water forty grams of sodium hydroxide may be dissolved and an equal quantity of dextrine, filtering if necessary. Twenty-eight grams of silver nitrate are to be dissolved in a small quantity of water and added by degrees at intervals. Complete solution readily takes place. Although the liquid contains less than one per cent of metallic silver it appears absolutely black, when diluted, red, by great dilution yellowish. With some specimens of dextrine the solution remains clear, with others it soon becomes a little turbid.

Perhaps the most interesting reaction which this solution shows, is that with disodic phosphate. A little phosphate is sufficient to throw down the whole of the silver although both solutions are alkaline. When a gram of phosphate in solution is added to 100 c.c. of silver solution the color becomes bright red, sometimes scarlet, and the whole of the silver is presently precipitated. This precipitate on the filter has a color like that of ruby copper, which color it retains during the first washing, but after a few hours' washing with distilled water the color changes to a deep Nile green and at the same time it becomes slightly soluble, giving a port wine colored solution. With more washing this solubility may disappear.

It is a general fact that all these forms of silver, however various their color, have both a body and a surface color and these two colors tend always to be complementary. The body color is that shown by the precipitate while still moist; it is also visible when a thin coat is brushed over paper, a coat so thin that light passes through it, is reflected by the paper and returned again through the film. But when a thick and opaque film is applied, the body color disappears and only the complementary surface color is visible.

So in the case of the precipitate by phosphate, when the substance resembling ruby copper is spread thickly on paper it dries with a bright green metallic surface color. But when the substance itself becomes green by continued washing it assumes on drying a dark gold or copper color, the surface color changing with the body color and maintaining its complementary relation. In observing these effects one is constantly reminded of certain coal tar colors, both by the great intensity of coloration and by the metallic surface color. I am

not aware that any other inorganic substance shows this resemblance to a similar extent.

These forms of allotropic silver have a great tendency to attach to themselves foreign matters. Although the dry substance has all the appearance of a pure metal it may contain as much as 8 or 10 per cent of organic matter which it is utterly impossible to detach. I have taken much trouble to eliminate this substance. In one attempt hot distilled water was forced through with the aid of a filter pump for over 100 hours without effect. The presence of this organic substance becomes evident when the metal is heated in a tube. A vapor arises which condenses into small brownish drops having an empyreumatic odor. The residue of bright white metallic silver, when dissolved in nitric acid, leaves behind black flakes of carbon. When the allotropic silver is dissolved in dilute nitric acid and the silver precipitated by hydrochloric acid, on evaporation a small residue of a yellowish gummy substance is obtained.

Analyses.—Four silver determinations were made of material rendered as pure as it was found possible to obtain it. Results—

No. 1	93·77	p. c.	Ag.
No. 2	94·27	"	"
No. 3	92·86	"	"
No. 4	96·64	"	"

Allotropic Silver obtained with Tannin and alkaline Carbonates.

Tannin (gallotannic acid) in alkaline solution reduces silver nitrate to metallic silver in the allotropic form. Tannin acts more strongly than dextrine and therefore does best with carbonated alkali, dextrine best with alkaline hydroxide, although either substance will produce the reaction with either form of alkali and, though less advantageously, with ammonia. Tannin with sodium carbonate gives a very perfect solution of silver, quite free from the turbidity that is apt to characterize the dextrine solution. The color of this solution is likewise very intense: one containing one per cent of silver is quite black, by dilution deep yellowish red. It has very much the same characters as the preceding, but is rather more stable. To obtain it, 24 grams of dry sodium carbonate may be dissolved in 1200 cc. of water. A 4 per cent solution of tannin is to be made and filtered, of this 72 cc. are to be added to the solution just named: of silver nitrate, 24 grams dissolved in a little water are to be added by degrees. Solution takes place almost instantly as each successive portion is added. The solution after standing a day or two may be decanted or filtered from a small quantity of black precipitate.

When the solution is treated with a very dilute acid, as for example, nitric acid diluted with twenty times its bulk of water, allotropic silver is precipitated in the solid form. It dries with a brilliant metallic surface color of a shade different from the foregoing and somewhat difficult to exactly characterize, a sort of bluish steel-gray.

I do not find that blue allotropic silver (in which is included the green and steel-gray varieties) can be reduced to any one definite type. On the contrary, its variations are endless. Slight differences in the conditions under which the solutions are formed or in the mode of precipitation give quite different products. For example, of ten products obtained with tannin and sodium carbonate in different proportions, several were easily and completely soluble in ammonia, some were slightly soluble and some not at all. Some specimens not at all soluble in water became so by moistening with dilute phosphoric acid: they did not dissolve in the acid but when it was removed they had become soluble in water. On other specimens phosphoric acid had no such effect. Some solutions are scarcely affected by acetic acid, others are partly precipitated, others almost but not quite wholly. The films spread on paper vary very much in their relations to light; some are readily converted into the yellow intermediate form, whilst others are very insensitive. The least sensitive specimens seemed to be those for which dilute nitric acid had been used as a precipitant. They had a steel-gray color. Precipitation by acetic acid seems to tend to a greenish metallic surface color and greater sensitiveness. Different specimens also vary very much as to permanency; this character is also affected by the amount of washing received: thorough washing tends to permanency.

In some way the blue, gray and green forms seem more closely related to the black or dark gray forms of normal silver, for they tend in time to pass into them, while on the contrary, gold-colored silver, if pure, tends with time to change to bright white normal silver on the surface, with dark or even black silver underneath.

Action of other Carbonates.

Tannin is capable of producing allotropic silver, not only in the presence of the carbonates of potassium and sodium, but also with those of lithium and ammonium and also with the carbonates of calcium, magnesium, barium and strontium. The action of the last named carbonate has been more particularly examined. It yields allotropic silver of a dark red color while moist, drying with a rich bluish green metallic surface color in thick films, in very thin films transparent red. It is probable

that the substances with which tannin produces these reactions would be further increased by investigation.

I have found some additional modes of production of these forms of silver, modes which are very curious and interesting. They are now being studied and will be reported on hereafter.

Nature of the "intermediate substance."

It has been mentioned in previous papers that when allotropic silver is converted into normal silver by the action of heat it passes through a perfectly well marked intermediate state. In this state it retains the gold-yellow color and high luster but none of the other properties of the original form. Oxidizing and chlorizing agents show nearly the same indifference as with ordinary silver. While allotropic silver is soft and easily reduced to powder the intermediate substance is hard and tough. When a glass rod is drawn over a film of allotropic silver it leaves behind it a white trace of ordinary silver. The intermediate substance shows no such reaction: the trace of a glass rod does not differ from the rest of the film and even hard burnishing produces no change in the color. Continued exposure to sunlight brings about the same alteration to the intermediate form and it takes place spontaneously with time.

At that time no explanation could be found as to the nature of the change. It proves however to be a passage into a crystalline form. Some films spread on paper were exposed to the action of very dilute solution of ferric chloride. It chanced that one of these films had undergone a partial change into the intermediate form; the unchanged portion was darkened by the ferric solution, while the portion that had passed into the intermediate form retained its bright gold-yellow color and luster, rendering it thus distinguishable. The figures which it exhibited were strikingly crystalline. One portion showed a foliated structure such as is formed by interpenetrating crystals, other parts showed ramifications with something of a plant-like form. Another part exhibited a sheaf of acicular crystals nearly parallel in direction, half an inch to an inch long and as fine as hairs. These appearances indicated with certainty crystalline structure. Other specimens have been obtained though none so well defined as that just mentioned which happened to be taken at exactly the right stage of spontaneous alteration to make the structure manifest. The alteration is not apparent to the eye as the color does not change.

This change to the crystalline condition does not seem to be peculiar to gold-colored silver. The blue form when gently heated in a tube becomes yellow. By continued heat it changes to white normal silver. A film on glass began to

change from blue to yellow at about 180° C. Light also produces this change on blue silver. The specimens obtained by different processes act very differently; some change with a few hours of strong sunlight, others require many days.

From what has been written in this and preceding papers it appears that allotropic and even soluble silver may be formed in a great variety of reactions. The reducing agents may be either a ferrous or a stannous salt or any one of a variety of organic substances of very different constitutions. From the solubility and activity of this substance and the parallelism which many of its reactions show to those of silver in combination, I have been disposed to think that silver in solution might, like silver in combination, exist in the atomic form. It is certain that up to the present time we have no positive knowledge of the existence of any element in the atomic form as a solid. We know that four or five metals are atomic in their vapors and that in iodine vapor at a certain temperature the molecules separate to atoms.

But it may be questioned whether we have not seen solid elements in the atomic form without recognizing them as such. There are forms of iron, nickel, cobalt and lead which exhibit very remarkable properties, properties that have been hitherto very unsatisfactorily explained. Lead tartrate reduced by gentle ignition in a nearly closed tube and allowed to cool and then shaken out into the air forms a stream of fire. The oxides of iron, nickel and cobalt reduced in closed tubes by hydrogen show similar properties. It is customary to explain this action by affirming that the metals are left in an extremely fine state of division. This explanation is not satisfactory. Sulphur, for example, is far more inflammable than any of the metals just mentioned and may be obtained in a state of exceedingly fine division, either by sublimation or by precipitation, but does not in consequence show any greater tendency to spontaneous inflammation. It seems more natural to suppose that these metals are reduced in the atomic form, and this view of the matter seems to be much strengthened by the following considerations.

The experiments of Ramsey, and of Heycock and Neville, cited in a previous paper, lead to the conclusion that in the case of a dilute solution of one metal in another the dissolved metal exists in the atomic form. But still more the experiments of Tammann on amalgams indicate that in these alloys the dissolved metal is atomic, and it is stated that Joule by distilling off the mercury from an iron amalgam found that the iron was left in a pyrophoric condition. The amalgam of manganese, carefully distilled, gives a pyrophoric powder. Chro-

mium amalgam, distilled in a current of hydrogen, gives a similar result if the temperature is not raised too high. The enormous affinity which these forms of metals exhibit for oxygen renders their study very difficult. It has not been before suggested that their activity is due to their being atomic, but this would seem to be a much more rational explanation than that of extreme division.* A broad distinction must of course be drawn between chemical and mechanical division: a substance may be atomic and yet appear in masses: may be in the finest mechanical division and yet be molecular or polymerized. Silver being a metal with a very low affinity for oxygen could not be expected to show in the atomic state the same inflammability as more oxidable metals.

In conclusion it may be said that there is much reason to suppose that elements may exist in the atomic form and that allotropic silver may present such a case. This is of course far from being proved and is offered only as a "working hypothesis." As such it may afford a useful aid in further investigations.

Philadelphia, April, 1891.

ART. LIX.—*Notes on the sub-marine channel of the Hudson River and other evidences of Post-glacial Subsidence of the Middle Atlantic Coast Region;* by A. LINDENKOHL. With Plate XVIII.

THE American Journal of Science of 1885† contained an article by the writer entitled "Geology of the Sea Bottom in the Approaches to New York Bay," in which a description was given of a remarkable depression in the sea bottom off Sandy Hook and an attempt was also made to account for the origin of this depression and to trace its connection with the geology of the adjacent coast region.

Professor Dana, who was the first to recognize the true shape of this depression and to direct attention to its existence by a map and reference in his "Manual of Geology," published in 1863, takes up the subject again in a recent number of this Journal,‡ treating of Long Island in the Quaternary with observations on the sub-marine Hudson River channel, and carefully

* M. G. Rousseau, in the new Encyclopédie Chimique, seems to entirely abandon the old view of extreme division and considers these forms to be allotropic and comparable with the allotropic forms of phosphorus, etc. Vol. iii, page 56.

† Vol xxix, pp. 475 et seq., also republished as Appendix No. 13, U. S. Coast and Geodetic Survey, Report of 1884.

‡ Vol. xl, pp. 425–437.

reviews the well-ascertained facts connected with this depression but on several points reaches quite different conclusions.

It is the object of the following pages to review the subject, and at the same time to introduce much information bearing upon it, which has accumulated since 1885, but has not appeared in print.

Description of the Sub-marine Hudson River Channel.—The sub-marine depression, to which reference is made in the preceding paragraphs, has the characteristics of a river channel unmistakably impressed upon it and it is recognized as the sub-marine continuation of the Hudson River channel. It is first noticed at a depth of twenty fathoms, about twelve statute miles southeast from Sandy Hook. Its course is nearly south until abreast of and eleven miles from Long Branch, where it has attained a depth of thirty fathoms in fifteen fathoms of water. Thence it begins to turn to the southeast and attains its greatest depth of forty-five fathoms when fifty-three miles from Sandy Hook, the banks rising on both sides of the channel to a height of fifteen fathoms within two or three miles. From here its depth begins to fall off until, at a distance of ninety-one miles from the Hook, the channel almost disappears with a depth of but forty-one fathoms in a surrounding bottom of thirty-nine fathoms depth. But, at a distance of ninety-seven miles, the channel begins to assume the character of a gorge or cañon, which character it maintains for a length of twenty-three miles, when it vanishes on the edge of the great continental plateau at a depth of about 200 fathoms. The average width of the river channel is about 1¼ miles, that of the gorge three miles with a greatest depth of 474 fathoms in about seventy fathoms of water.

The bottom of the river channel and cañon as well as their slopes consist of a bluish slate-colored mud or clay with a fine sandy grit. This mud bottom extends for a considerable distance on both sides, north and south of the cañon along the brink of the continental plateau.

In trying to account for the existence of the upper part, or river portion, of the channel, it was assumed to be the result of fluvial erosion and to imply a subsidence of about 210 feet at a comparatively recent geological time, subsequent to the glacial period. The existence of the gorge was believed to imply a much greater subsidence than that of the upper channel, a subsidence not far from 1200 feet (200 fathoms). No attempt was made to fix its geological date beyond the statement that its fiord-like shape favors the supposition that it existed as an elevated channel during a part at least of the glacial era, but that it must have sunken below the level of the

ocean at the time when its feeder was yet an actual river channel.

The presence of clay on the bottom of the channels and on the slopes, and its absence elsewhere, was assumed to furnish proof for the assumption that this clay was not a mere superficial covering, but that it is formed *in situ* and gives indications of strata in correlation with the Tertiary exposed towards the northeast at Gay Head, as well as with that of New Jersey, bearing west.

The first one of these propositions, the one which accounts for the sunken river channel, is the most important and perhaps the most vulnerable one, and requires proof of the following corollaries:

1st. The shape and dimensions of the channel must accord with those which should be assigned to a hypothetical river of the size of the Hudson.

2d. Tidal and other currents now in existence cannot have produced the channel.

3d. A similar subsidence which must not necessarily be of the same amount, must be proved for the nearest rivers to the south, for the Delaware, Susquehanna (or Chesapeake) and Potomac. (The rivers to the north may be left untouched since Professor Dana has investigated the subject and recorded the results in his paper on Long Island Sound in the Quaternary, etc., mentioned above.)

4th. It must be shown that diluvial deposits do not lie conformably on the surface of these channels but are eroded by them, and all deposits found in the channels must be of alluvial character.

1. *Size and shape of the Sub-marine Hudson River Channel.*—The breadth of the channel is about one and a quarter miles, about the same as that of the river above the Narrows. From New York City to the Dunderberg the channel is about three-quarters of a mile wide. These dimensions tally well with the conditions expected from an ordinary tidal stream, i. e. increased capacity with nearer approach to sea. The main slope of the banks is 1°. This is less than we expect of living rivers, but we should take into consideration that, apart from currents, the corrasive action of sea water is constantly engaged upon the work of destruction. It is rather a matter of surprise to those who are familiar with the little power of resistance of clay banks in sub-aerial exposure when unprotected by gravel ledges or turf, that such banks should be preserved at all under the sea. No special reason can be assigned for the peculiarity that the river should first flow fifteen miles to the south before turning east unless we assume that it follows the fashion set by its neighboring rivers, the

Delaware, Susquehanna, and Potomac, which all follow a southern and anticlinal course before they take up the straight road to sea. This uniformity in behavior of these four rivers points to a common cause, and a slight tilting of the Atlantic plain in a north and south direction suggests itself as the readiest way to account for the southern deflection of the rivers.

Effects of existing currents on the sub-marine channel.—The following table, giving the mean maximum strength of both sets of tidal currents in nautical miles, has been compiled from the latest observations for the purpose of testing the ability of those currents to create channels outside of the Sandy Hook bars.

Table of Tidal Currents in the lower New York Bay.

	Mean Maximum Ebb.		Mean Maximum Flood.	
	Surface.	Bottom.	Surface.	Bottom.
Narrows	1·9	0·9	1·3	1·0
Fourteen Foot Channel	1·9	0·9	1·6	1·0
East Channel	2·2	0·9	1·6	0·8
Swash Channel	2·1	0·9	1·8	1·2
Main Ship Channel	2·3	0·8	1·8	1·2
Outer East Channel	2·1	1·2	1·6	0·9
Gedney's Channel	2·3	0 9	1·8	1·0
South Channel	2·2	0·2	1·4	0·7

It will be seen from this table that the ebb current is the strongest surface current and maintains its velocity (1·9 to 2·3 knots) until the outer bars have been passed. But its strength at the bottom of the channels is less than half that of the surface currents, and less than that of the flood current. The flood current is essentially a deep current and retains at the bottom, in spite of friction, nearly two-thirds of its surface velocity; it is the flood current's speciality to attend to the scouring business. But we cannot realize that a current which has but 1·2 nautical miles velocity at the places where it must be supposed to exert its greatest strength, can have the power to scoop out a channel forty-five feet deep and only a mile wide at a distance of fifty miles from the coast. At the same time the opinion is well-founded that the submarine channel is the principal passage way for the tide to and from New York Bay, and that this almost ceaseless flow tends to keep the channel clear from encroachments, especially by that formidable bank, the Cholera Bank on the New York side of the channel. Often the tides may be reinforced by high seas produced by continuous easterly winds off New York; and although such high tides are known to be very destructive along the whole coast from Atlantic City to Fire Island, we have no reason to believe their effect to extend to greater depths than fifteen fathoms or to the depth of our channel.

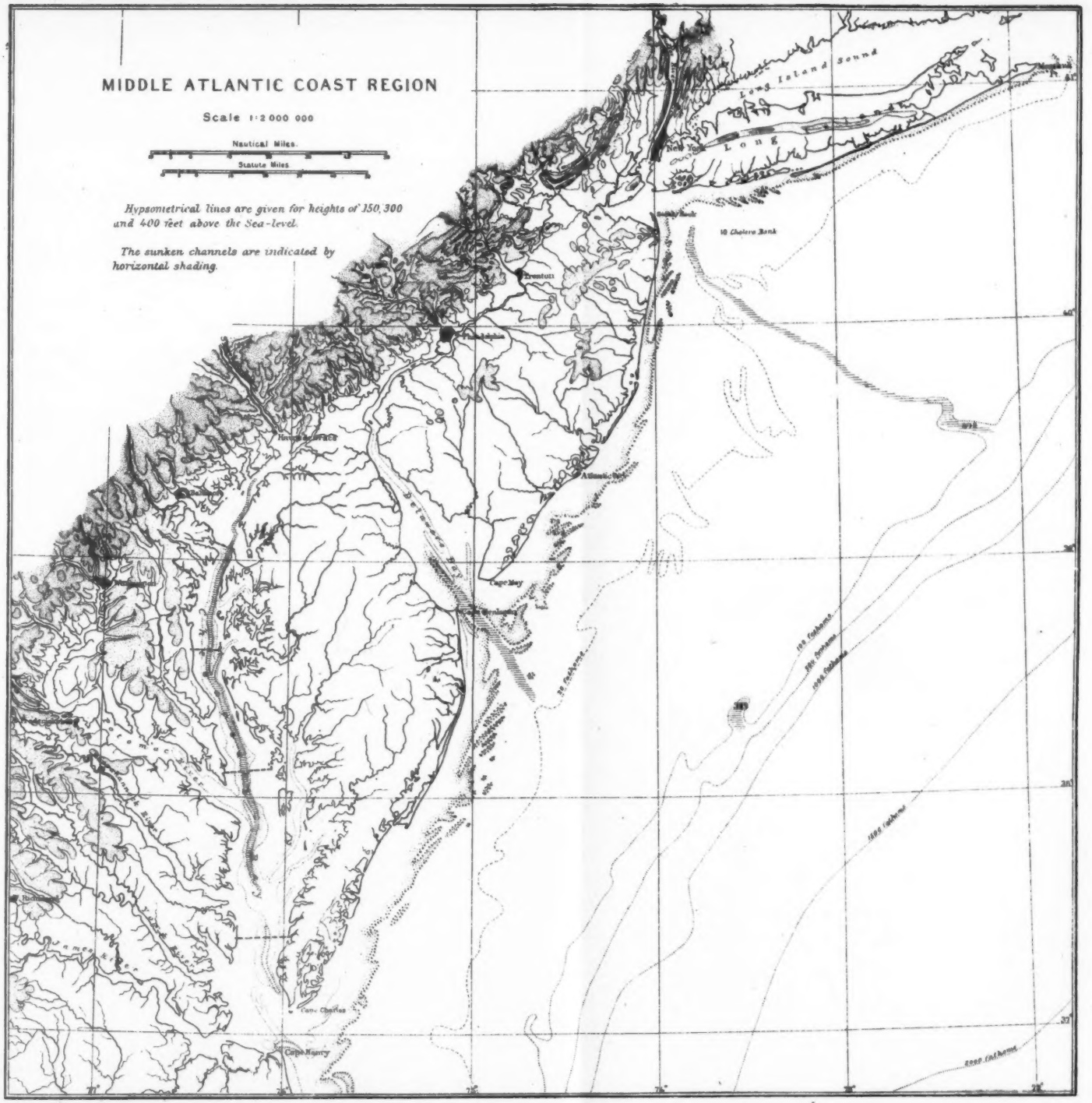
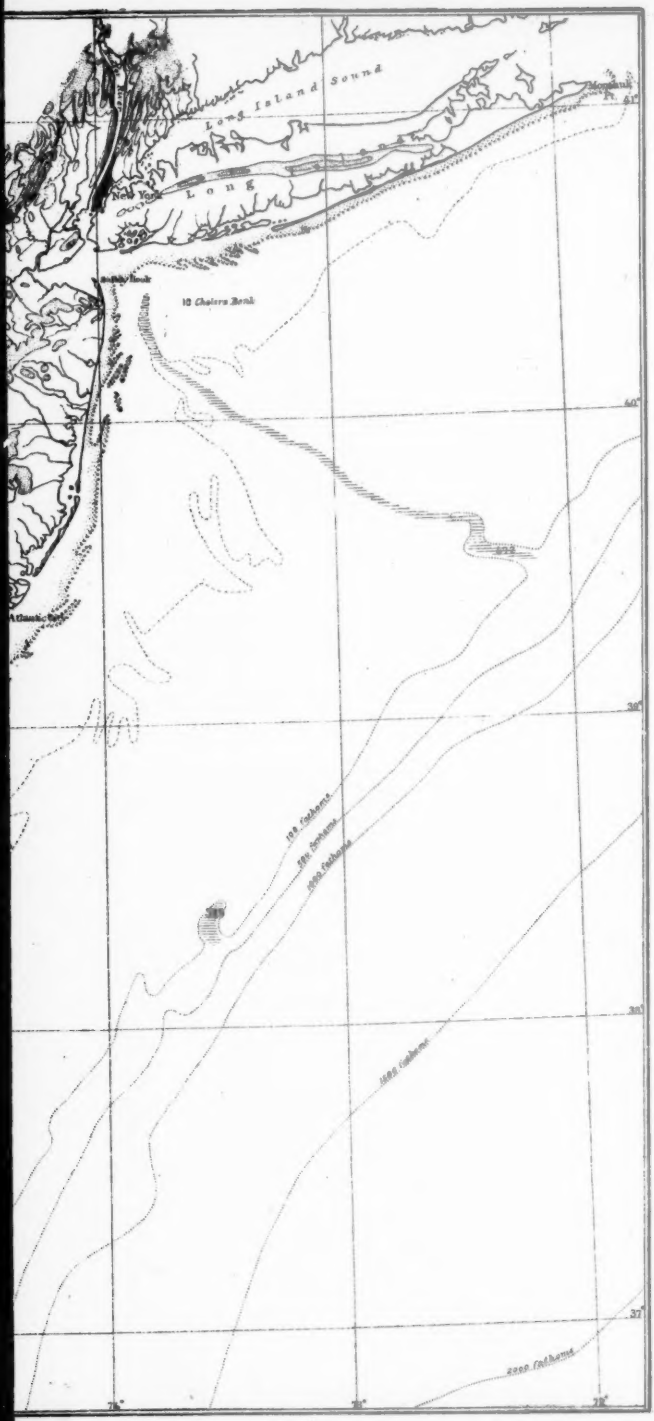

MIDDLE ATLANTIC COAST REGION
Scale 1:2 000 000
Nautical Miles.
Statute Miles.
Hypsometrical lines are given for heights of 150, 300 and 400 feet above the Sea-level.
The sunken channels are indicated by horizontal shading.
Long Island Sound
Long
New York
10 Cholera Bank
Trenton
Philadelphia
Atlantic City
Baltimore
Washington
Richmond
Cape Henry
Cape Charles
100 fathoms
1000 fathoms
2000 fathoms

Plate XVIII.


Long Island Sound
Montauk Pt.
New York
Long
Long Island
Cholera Bank
Atlantic Ocean
100 fathoms
500 fathoms
1000 fathoms
2000 fathoms
40°
39°
38°
37°
74°
73°
72°

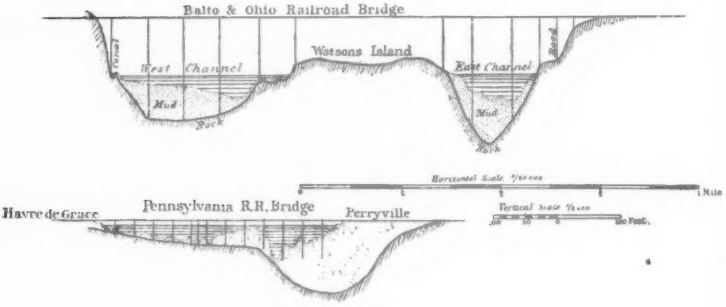
Sunken River Channel in Chesapeake Bay.—As stated above, if the theory of a recent subsidence of the Hudson River is to be successfully maintained, a similar subsidence must be proved for the Delaware and for Chesapeake Bay. It must be confessed that for many years we have been searching for sunken channels for those bays without finding them; we were looking for them in a wrong direction, outside of the bays, instead of inside. We supposed Cape Henlopen and Cape Henry occupied relative positions to those channels analogous to that of Sandy Hook to the sunken Hudson River channel; we took the Coast-line as our line of departure instead of taking the *Fall-line*. This line which is easily identified by the site of New York, Trenton, Philadelphia, Havre de Grace, Baltimore, Washington, etc., separates two widely different geological regions, the region of crystalline and Triassic rocks to the north and west from the stratified clays and gravels to the south and east, and it must be assumed that any seismic disturbance would affect these two regions unequally and the coastal plain to a greater extent than the Piedmont region.

Now, the *sinking* of the land to the extent of 100 feet, let us say fifty feet, would hardly affect the physiography of those parts of the country above the hypsometrical line of fifty feet, but all land below this level would be appropriated by the waters and reached by the tides; rivers with low shores would be converted into bays or estuaries, those situated in rising ground would have the lower parts of their valleys flooded. It now remains for us to examine Delaware and Chesapeake Bays for traces of deeper and narrower channels than those which can be accounted for by existing conditions.

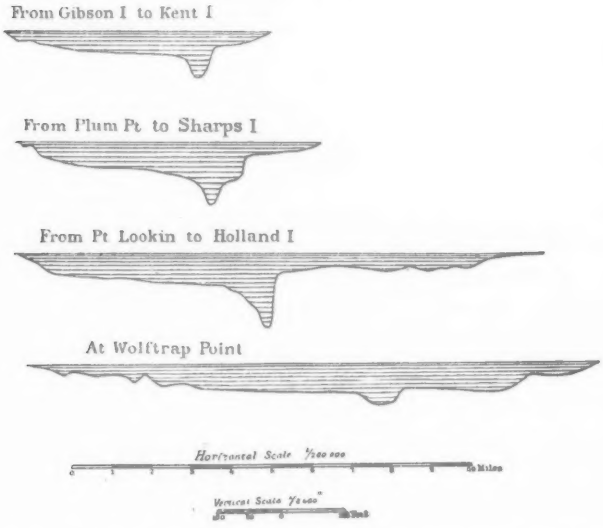
Passing Delaware Bay, for reasons which will be explained farther on, and turning to Chesapeake Bay, we readily find, upon examining the soundings, a narrow and deep inner channel which can be traced nearly through the entire length of the bay, from the mouth of Bush River to that of the Rappahannock, a distance of 120 miles. In an average width of the bay of ten miles, this channel commences with one mile's breadth in its upper part, increasing to two miles near its southern limit. The descent of the bottom of the bay is very gradual from the shore until the depth of eight fathoms is passed, when the bottom abruptly plunges to the depth of about twenty fathoms (from fifteen to twenty-six fathoms). The bathymetrical line of forty-eight feet may then be taken as the limit of this inner deep channel. We subjoin four cross-sections of the bay, taken about thirty-five miles apart. It will be seen that the areas of these sections are gradually increasing,

going down the bay and that the last one, that off Wolftrap Point below the Rappahannock, is the largest, although the

Cross Sections of the Susquehanna River



Cross Sections of Chesapeake Bay


From Gibson I to Kent I
From Plum Pt to Sharps I
From Pt Lookin to Holland I
At Wolftrap Point
Horizontal Scale 1/160 000
10 Miles
Vertical Scale 1/4000
60 30 0 100 Feet

deep cut in the bottom is missing. It does not matter what particular curve we accept as a type of cross-sections of tidal

streams, whether an ellipse or parabola, the cross-section at Wolftrap comes nearer to it than any of the preceding ones and no combination of circumstances, no shifting or turning of channel can satisfactorily explain, as a purely tidal phenomenon, the existence of the deep incisions at the bottom of the cross-sections. We are forced to conclude that these incisions are due to pre-existing conditions, that they show the former channel of a river at a time when the whole region lay about forty-eight feet higher than at present, when Chesapeake Bay did not exist, but when the Susquehanna was at least 150 miles longer than at present (rather more than the submerged Hudson) and gathered upon its way to the sea the waters of the Patuxent, Potomac and Rappahannock. The reason that we cannot trace the channel farther up the bay than Bush River and to the mouth of the Susquehanna is, no doubt, owing to the fact that the Susquehanna has filled up the upper part of its old channel, for which it has no further use, with its sediment; and the borings to a depth of 140 feet at Fishing Battery, below Havre de Grace, through alluvium, which Mr. McGee reports,* quite favor such a supposition. As stated above, the channel disappears below the mouth of the Rappahannock with a depth of about fifty feet. I am not prepared now to answer the question, whether the bar and actual end of the old river is here or whether there is but a temporary interruption of the channel by subsequent deposits from rivers emptying into the bay. The answer is not material to the present inquiry. The river channel appears to have hugged its eastern shore, which in several places appears to have risen into bluffs, from 15 to 25 feet high, while the western shore was low and marshy. The soundings in the bay are not the only indications of a depression; they can be found everywhere along the shores of the bay, even by a mere inspection of the charts. It is entirely beyond the ability of the present sluggish streams to have eroded their channels to the great width which is so characteristic of the lower part of all streams entering the Chesapeake. The absence of deltas and bars at the mouths of the rivers, the almost total absence of drainage-area for a long strip of the western shore of the bay above the Rappahannock, all are suggestive of subsidence, in fact have been commented upon in this direction by Mr. McGee in his exhaustive study of the Geology of Chesapeake Bay.† On more than one occasion he speaks of the drowned rivers of the bay.

There is another way in which we may arrive at an estimate of the probable amount of subsidence. The profile given (p. 494) of the Susquehanna River at the crossing of the Baltimore and

* Seventh Annual Report U. S. Geological Survey, p. 580.

† Seventh Annual Report, U. S. Geological Survey, pp. 537–646.

Ohio Railroad bridge above Havre de Grace, kindly furnished by Mr. Chas. F. Mayer, the President of the road, shows a considerable layer of mud intervening between the bottom of the river and the rocky granite floor. This layer has a thickness of fifty-nine feet in the west, and over seventy feet in the east channel. The river would most certainly not have cut a channel into one of the hardest of rocks if there had not existed, at some time, a physical necessity for it, and the amount of filling or "packing" of mud enables us to estimate the depth of the river at that time. Assuming the discharge to be stationary, we find that, supposing the mud to be removed, the river could be lowered forty-three feet and yet find sufficient space for the passage of its waters. The next profile (p. 494) is from the crossing of the Pennsylvania Railroad at Havre de Grace, about one mile to the southward of the B. & O. R. R. bridge. This profile was obtained from Mr. G. B. Roberts, President of the Penna. R. R. It shows the greatest depth of mud, 113 feet under the wharf at Perryville. It would appear then that the channel of the river ran very closely to its eastern shore which was then several hundred feet farther inland. The rock is stated by Mr. McGee to dip under the level of the river about one-quarter of a mile from the railroad bridge. A similar calculation for the level of the river with the rocky floor for its bed, instead of the muddy bottom, gives fifty feet below the present surface. These two estimates taken in connection with the result of borings at Fishing Battery mentioned above, would appear to prove that at the time when the level of the Chesapeake was forty-eight or fifty feet lower with respect to the land than at present, Perryville and not Port Deposit, was at the head of tide and that strong currents swept down the Susquehanna past and on both sides of Watson Island, plowing into the clays of the coastal plain to a depth of ninety feet or more.

The Potomac being a tributary of Chesapeake Bay, we should naturally expect indications of a sinking of the land at the head of tide, similar to those of the Susquehanna. An examination of several profiles of the river at the Free Bridge in Georgetown (the former Aqueduct Bridge, built about 1840) shows the excavation of the channel to be of quite different shape from that of the Susquehanna; it is flat at the bottom and only reaches to the depth of thirty-five feet from the surface. There was considerable "packing" by mud before the bridge was built, about thirteen feet thickness on an average. The cross-section of the river was considerably curtailed by the construction of the bridge. The river has tried to regain its former status and nearly succeeded in this effort, by removing the greater part of the

mud at its bottom. Supposing all the mud and artificial obstructions to be removed, the river could stand a lowering of its level of but eleven feet. Judging from surface exposures, the rocks at the bottom of the river are frangible or disintegrated gneiss, which is certainly less obdurate than the granite of the Susquehanna gorge, hence we have to conclude that the dislocation here is scarcely one-fourth of that of the Chesapeake Valley. At the site of the proposed Memorial Bridge, 1000 feet east from Easby's Wharf, rocky bottom is found at a depth of forty-four feet below the surface of the river; the stratum of mud here is about fifteen feet thick. At the Long Bridge, rock bottom has not been reached by boring or pile driving, and hard bottom in the Washington channel is seventy-one feet below the surface under a layer of sandy mud of sixty-nine feet thickness. The Georgetown channel has no mud at its bottom but runs over a hard bed of gravel and clay. A subsidence along the valley of the Potomac below Washington, inferior to that of Chesapeake Bay, is attested by the bay-like expansion of all the affluents at their mouths.

Subsidence in Delaware Bay.—I have not had the necessary time nor data at hand to make a similar inquiry about probable subsidence in Delaware Bay. In fact, we know Delaware River and Bay to have much stronger currents and to carry a greater amount of coarser sediment than Chesapeake Bay, and are prepared to find the traces of a former higher level less distinctly preserved. Nevertheless, we can trace a deep channel from the ocean into the middle of the bay where it is apparently choked off by alluvial deposits which fill up the entire upper part of the bay, leaving just enough room for the river channel. This "blind channel" has a depth of from twenty-two to thirteen fathoms, and is separated from the main river channel by shallow banks. The ebb-channel in actual use by the river has but a depth of three and one-quarter fathoms in its shoalest reaches. A comparison of our recent surveys with those made about fifty years ago proves that the high-water line, on the New Jersey side at least, has receded about one-eighth of a mile in the lower bay; but it would be rash to make subsidence responsible for this result. A comparison of the hydrographic surveys made about the same respective dates shows that there has been a great deal of shoaling going on in the interval, and it is not impossible that this shoaling has produced a disarrangement of the tidal elements, a retardment accompanied by an increase in amplitude which would show its effects on the high-water line.

Time of subsidence.—The evidence of a subsidence of the coast of New Jersey during the past century and yet in progress, collected by the late Professor Cook, must be consid-

ered as the main support of the theory which accounts for the existence of the submarine channel of the Hudson by submergence. In order to approximate the time of commencement of subsidence we have to take the evidence afforded by the latest Quaternary deposits. According to Mr. McGee* the clay terraces on which the city of Washington is built and which are supposed to be cotemporary with the first glacial invasion, indicate a submergence of about 150 feet during the period of their deposition. Hence it appears that the Lower Potomac and Chesapeake Bay with their depressed channels are of more recent origin. The borings at Fishing Battery cited above, which went to a depth of 140 feet and brought nothing to light older than alluvium, teach us that the deep channel of the Chesapeake must be of more recent date than any of the Tertiary and Quaternary deposits about the head of the bay.

The submarine border of the coast.—Returning to the subject of submarine channels, it has to be stated that diligent search has thus far failed to discover indications of such for either Chesapeake Bay or Delaware Bay, with the exception of a deep *cul-de-sac* of 396 fathoms inside of the bathymetric line of 100 fathoms, occupying nearly the same relative position to Delaware Bay as the cañon described above does to the Hudson.

In studying the geological changes in the sea bottom off the Middle Atlantic States, a remarkable fact should not be lost sight of. The sea bottom intervening between the submarine Hudson river channel and the coast of Long Island is characterized by its great regularity and smoothness, which can best be explained by assuming a gradual subsidence or an adjustment by superficial deposits. The bottom between the channel and the New Jersey coast, on the contrary, is distinguished for its ruggedness; great irregularities in the soundings give indications of shallow ridges and of cross channels, which go to prove that there was a periodical retrogression of the coast line, and that the sea keeps the conquered territory in very much the same condition in which it was found.

Greensand at the sea bottom.—The specimens of bottom collected during the recent survey of the approaches to New York, of which there are several hundreds at hand, show considerable quantities of black grains, described black specks on the charts, mixed up with the sand and mud of the entire region from Cape May to beyond Montauk Point; it is only in the mud of the gorge and of the deeper part of the continental slope that they are either scarce or missing. They are of spherical shape, of jet black luster, of brown color when fractured, and vary in size, with the fineness of the sand or mud, from the

* This Journal, vol. xxxv, May, 1888.

size of a pin-head to microscopical dimensions. They were evidently not composed of hornblende, and I hesitated to pronounce them greensand, which material Mr. Pourtalès, in 1869, reported to exist in the sands off Long Branch and Rockaway Beach.* Mr. McGee was kind enough to have an examination made in the laboratory of the U. S. Geological Survey and informs me "that the black grains are, as Count Pourtalès supposed, glauconite. The mineral seems to have undergone a curious alteration and the grains were polished through attrition and partly through chemic and mechanical alteration akin to that of nodulation, but the density, optical properties, hardness, etc., of the broken grains are identical with the like properties of New Jersey greensand from the Cretaceous and Eocene." It was an open question with Mr. Pourtalès whether these grains were washed out to sea from the marl beds of New Jersey or belonged to beds cropping out at the sea bottom. In view of the great extent of ground over which these grains are spread, and the great distance from the New Jersey coast, close to Montauk Point for instance, the first supposition can no longer be maintained; they must be treated either as belonging to marl beds laid bare by the sea or as the remnants of such which have been destroyed by the sea.† Whether these beds were Cretaceous or Eocene strata is a question which probably can only be decided upon paleontological grounds, but the preponderant strength of the Cretaceous on the mainland certainly speaks in favor of the latter having supplied the greatest amount of greensand grains to the ocean's bottom.

March 26, 1891.

ART. LX.—*Are there Glacial Records in the Newark System?* by ISRAEL C. RUSSELL.

SEVERAL expressions of opinion have been published by geologists in this country, respecting the existence of glaciers along the Atlantic border during the deposition of the rocks of the Newark system. It has been recently stated by Prof. J. D. Dana,‡ that this period "ended in a semi-glacial era, as is

* Appendix, No. 11, U. S. Coast Survey, Report of 1869, also Petermann's Geogr. Mittheilungen, vol. xvi, pp. 393–398.

† Quartz sand and pebbles, and greensand (perhaps hornblende) seem to be the only minerals which preserve their integrity in moderate depths at the bottom of the ocean; feldspar and mica are rarely found, and the so-called "mud" consists very often of the finest sand with some mica flakes, and with a just sufficient admixture of clay to produce cohesion.

‡ This Journal, III, xl, 1890, p. 436.

admitted by all who have studied the beds." As there is possibly not the unanimity of opinion among those who have studied the Newark system, that is suggested in the line quoted; it may be well to glance at the evidence on which those who consider that the period referred to contains glacial deposits or ended in a semi-glacial era, base their conclusions.

In describing certain coarse deposits on the eastern border of the Deep River area of Newark rocks in North Carolina, W. C Kerr* suggested that they indicate a sub-Newark glaciation: but these beds are considered as post-Newark by W. M. Fontaine.† A similar remark was made by N. S. Shaler and W. M. Davis,‡ in reference to the origin of the coarse conglomerate of the Newark system in the Connecticut valley. It has also been stated by J. D. Dana§ that the Connecticut valley had its violent floods during the Newark period, which may have been enlarged by the waters and ice of a semi-glacial era. But the most extended discussion of the possible glacial origin of certain coarse deposits in the Newark system has been made by W. M. Fontaine.† The arguments he advances are based on the following considerations:

First. The presence of coarse conglomerate and breccias. *Second.* Absence of fossil mollusks, radiates, etc. *Third.* Unexplained phenomena in the drainage and relief of the Appalachians, which are supposed to have been initiated by glaciers. *Fourth.* Extinction of the fauna and great change in the flora of the Atlantic border in the interval between the Newark and Cretaceous periods.

The evidence of glaciation according to Dana‖ "consists in thick deposits of stones and bowlders in which occur masses two to four feet in diameter, and therefore such as only ice could have handled and transported. They are situated along the west side of the area in Virginia, Maryland and New Jersey (where the dip of the Jura-Trias [Newark] beds is westward) and on the eastern in Connecticut and Massachusetts (where the dip is eastward).¶ Fontaine has found in Virginia and Maryland that they are the *later beds* of the formation." Exposures of Newark rocks north of Amherst, Mass., containing bowlders three to four feet in diameter are referred to, and Edward Hitchcock's conclusion that they are "the upper beds" of the series, cited. Coarse deposits near East Haven, Conn., are also mentioned.

* Rep. Geol. North Carolina, vol. i, 1875, p. 146.
† This Journal, III, xvii, 1879, p. 34.
‡ Illustrations of the Earth's surface, Glaciers. Boston, 1881, pp. 95, 96.
§ This Journal, III, xvii, 1879, p. 330.
‖ This Journal, III, xl, 1890, p. 436.
¶ Note in this Journal, III, xli, 1891.

Before examining the evidence, let us endeavor to determine what facts should be looked for, in case glaciers did invade the area in which Newark rocks were being deposited.

Preservation of Glacial Records.

All records of glaciation not buried beneath subsequent deposits, would certainly be destroyed by subaërial decay and erosion, during such a lapse of time as has intervened between the Newark period and the present day. Exception to this conclusion may, perhaps, be found in the changes which glaciers make in the drainage and topography of a region, but this matter is as yet little understood. Among the direct evidences of glaciation which might be preserved for indefinite ages, under suitable conditions, the following may be enumerated:

First. Smoothed and striated rock surfaces, if buried beneath fine sediments, might be preserved in their original condition; or casts of them might be taken, in the same manner that casts of footprints, showing the most delicate markings, have been preserved in great abundance in the Newark rocks themselves.

Second. Bowlders, smoothed, faceted and striated by glaciers (similar markings are also produced by river ice), might retain their records for indefinite periods, especially if they were imbedded in fine sediments or cemented by calcareous or other infiltrations.*

Third. When glaciers enter an estuary or a lake, moraines are deposited in unassorted or but imperfectly arranged, heaps about their extremities. The distance from the shore to which these deposits may be carried depends on the size of the glaciers and on the depth of the water they enter. A shallow estuary or lake could offer but feeble resistance to the advance of glaciers and might have moraines deposited widely over its bottom. On the other hand, glaciers entering a basin in which the water is as deep as the ice streams are thick, would have their advance checked abruptly and the moraines deposited would be confined to the borders of the basin; but scattered bowlders might be carried to great distances on floating ice. Should glaciers plow their way into shallow basins in which fine sediments were being deposited, it is evident that the beds beneath them might be greatly disturbed, while contortions would appear in adjacent strata owing to the unequal distribution of the load imposed on them.

* Since writing these pages, a paper has appeared by Dr. Hans Reusch, on "Glacial striæ and bowlder-clay in Norwegian Lapponie from a period much older than the last ice-age." *Norges geol. undersögelse aarbog for* 1891, in which descriptions are given of striated rock surfaces protected by morainal material containing striated and faceted bowlders. These records are thought to be of Cambro-Silurian age.

Fourth. The effect of a glacial period on animal and plant life has received much attention, but what records of the presence of glaciers at any special time might be expected in contemporaneous fossils, is still indefinite. A glacial period is usually considered as a cold period (the presence of local glaciers in mountainous regions, however, does not imply an Arctic climate). If a cold period followed a warm period, it is to be expected that migrations of fauna and flora would follow. Alternations of warm and cold periods would probably be accompanied by the extinction of many types. That changes of this character did take place during the Pleistocene glacial epoch, is apparently well established.

On the other hand, we know that luxuriant floras exist on the margins and even on the surface, of living glaciers of vast extent, which are not essentially different from plants of the same species growing at a distance from all perennial ice. In the present state of knowledge, it does not seem possible for paleobotanists to designate any group or assemblage of fossil plants which might not have flourished in proximity to glaciers.

While glaciers may be surrounded by a mild atmosphere, congenial to plant life, the waters into which they discharge either directly or after melting, are cold and could not be inhabited by animals characteristic of temperate or tropical climates.

It is to be expected, therefore, that sediments laid down in water into which glaciers are discharging could only contain the records of faunas, exclusive of land animals, characteristic of cold climates.

Weight of the Evidence.

Having in mind what records might reasonably be expected to occur in the rocks of the Newark system, providing glaciers had assisted in their deposition, let us see what the facts are:

First. No smoothed or striated rock surfaces either in the Newark system itself or on the floor on which it rests, have ever been found.

Second. No glaciated bowlders have been observed in the system.

Third. No scattered bowlders or large rock fragments indicating iceberg drift, have been found in the off-shore deposits.

Fourth. No Newark fossils indicating a cold climate, even in a remote way, have been discovered.

The paucity of molluscan life has been advanced as indicating Arctic conditions. It is well known, however, that mollusks and many other forms of life, inhabit the seas of northern regions close to where glaciers discharge. Shells occur also in

Pleistocene glacial clays. The evidence that glaciers are not necessarily accompanied by an extermination of mollusks in adjacent waters, is abundant. Besides, the hypothesis that mollusks are absent from the Newark sediments on account of glacial conditions, does not stand alone; other explanations of the same phenomena with many facts to support them have been advanced.

I have previously suggested that the great numbers of reptiles, some of them of gigantic size, the bones and foot prints of which occur in the Newark rocks, could not have lived in an estuary or lake into which glaciers were discharging and on which icebergs floated. Cold-blooded animals at the present time among which we must look for the nearest living allies of the "foot-print animals," are confined to warm regions; and there is no reason to suppose that this law of nature was reversed during the Newark period. The swarms of reptiles, both great and small, that lived at that time must have required an abundant food supply; this implies that the shores they haunted were more like those of Florida than those of Greenland at the present time.

The fossil plants of the Newark are Araucarians, ferns, equiseta and cycads. If these have any bearing on the question of climate they indicate sub-tropical and not Arctic conditions; although there is no reason why they might not have grown in proximity to glaciers.

In regard to the marked modifications in the flora and the extermination of the fauna of the Newark period, before the deposition of the next succeeding formation, referred to by Fontaine, we know that such changes have occurred at the close of each important division of geological history and may be accounted for in most cases by the imperfections in the records. Breaks in the life records invariably accompany breaks in stratigraphy. The assumption that these changes at the close of the Newark period were due to glaciation, is making an exception to a general rule, without facts to support it.

The Coarse Deposits.

It has been stated by Hitchcock and Fontaine, that the coarse deposits of the Newark system belong at the top. These statements lead Dana, as already cited, to the conclusion that the Newark period ended in a semi-glacial era. If the coarse deposit were confined to the top of the series, which as I shall show below, they are not, it would scarcely follow that they marked the close of the period, for the reason that deep erosion has unquestionably taken place.

But the coarse deposits are not confined to the top of the series. Wherever the base of the system is exposed it is almost

always found to be a coarse conglomerate or breccia. This deposit has been brought to the surface in several instances by faults and its character is well known. Coarse conglomerates occur especially on the east border of the Connecticut Valley area and on the west border of the New York-Virginia area and the detached areas in Virginia and North Carolina which fall in line with it. The beds are not continuous, but occur as local deposits. Where the coarse material is thickest, it grades into fine material both along the strike, that is along the shores against which it was deposited, and in a direction at right angles to the shore. Toward the center of the area of deposition, the coarse beds become fine and overlap or interdigitate, with fine off-shore sediments. As the upheaval of the system has not been uniform, erosion has cut far deeper in certain localities than at others. Thus, in northern New Jersey the Newark rocks are known to be not less than three or four thousand feet thick, while in the prolongation of the same area in Maryland, the thickness is certainly much less. The present base-level of erosion has cut deeper into the system at the south than at the north; yet all along the ancient shore, joining the two extremes, coarse deposits occur from time to time. The evidence points definitely to the conclusion that coarse deposits occur at all horizons from the bottom of the system up to the highest beds that now remain. If these beds are glacial deposits, then glaciers must have existed throughout the deposition of the system.

The coarse deposits along portions of the borders of the various Newark areas are always of local origin. They are not heterogeneous accumulations gathered from a broad area, as might be expected if they are of glacial origin, but have been derived from terranes in the immediate vicinity of where they now occur. The material forming the large bowlders in particular, may invariably be found *in situ*, near at hand.

No contortion of the fine sediments adjacent to or interstratified with the coarse deposits have been observed, such as would result from the extension of glaciers into the basins in which the fine sediments had been accumulated, or from the superposition or moraines upon them. On the contrary, the phenomena noted at many localities are fully explained on the assumption that the strata both coarse and fine, were deposited contemporaneously as water-laid beds.

The fine deposits intimately associated and even interstratified with the coarse deposits, are frequently ripple-marked, sun-cracked, and contain rain-drop impressions and the footprints of animals, at many horizons; thus showing conclusively, that the water bodies in which the strata were spread out were shallow. There is, therefore, no reason why glaciers

entering the basin should have halted at the immediate shore, and deposited their loads. Besides, the coarse deposits are not of the heterogeneous character typical of morainal accumulations, but are stratified and cross-bedded. It has been stated by those who claim that the deposits in question are of glacial origin, that they contain bowlders three to four feet in diameter, and therefore such as only ice could have handled and transported My own observations have shown that bowlders either rounded or sub-angular, of the size indicated are not rare. Huge angular masses of rock like those to be seen on nearly every Alpine glacier, however, never occur. Those of my readers who have followed the Appalachian rivers south of the southern margin of the drift, will remember many instances where streams are encumbered with bowlders of even larger size than those mentioned above. These are being swept along by every flood, and did the streams empty into lakes, would be deposited in shore conglomerates. It may be remarked also, that river ice might have assisted in the movement of the Newark bowlders, without supposing the existence of glaciers.

After reviewing the voluminous literature relating to the Newark system and personally examining nearly every area occupied by it, I fail to find any evidence to support the hypothesis that glaciers assisted in its deposition. That there may have been glaciers on the Appalachians previous to or during the Newark period, is within the bounds of possibility, but as yet there is no evidence in this connection on which to base an opinion; we can only say that if they were present during the period under discussion, they did not reach the estuaries in which sediments were being deposited.

Washington, D. C., Feb. 15, 1891.

Art. LXI.—*A reply to Professor Nipher on "The Theory of the Solar Corona"*; by F. H. Bigelow.

In the Report of the Washington University Eclipse Party, on the Total Eclipse of the Sun, January 1, 1889, Professor Francis E. Nipher makes a criticism of my paper on the Solar Corona published by the Smithsonian Institution, 1889. The theory which I have proposed is of itself sufficiently technical in a mathematical sense, not to be burdened with an inaccurate or irrelevant criticism, and I therefore wish to make the three following observations on Professor Nipher's Report.

1. The mathematical *non sequitur* of the work on pages 22, 23 is so obvious as to need no special comment. I am informed that the equation for the line of force which should follow

from the equation of the force at any point, $F = \frac{\varphi a^3}{R^3}(3\cos^2\omega+1)^{\frac{1}{2}}$, is not what he intended to have appear. Instead of reading, $N = \frac{2\pi\varphi a^3}{R^3}(3\cos^2\omega+1)^{\frac{1}{2}}(1-\cos\omega) + \varphi\pi R^2\sin^2\omega$, it should read, $N = \frac{2\pi\varphi a^3}{R}\sin^2\omega + \pi\varphi R\omega^2\sin^2\omega$. The equation in its corrected form gives two terms, the first, $2\pi\varphi a^3\frac{\sin^2\omega}{R}$, which is identical with that employed by me in my paper, and the second, $\pi\varphi R^2\sin^2\omega$ which Mr. Nipher ascribes to the action of a uniform field of force surrounding the Sun.

2. At this place the ways part, and the irrelevant criticism begins. The equation in N with one term represents a certain condition of things; the equation in N with two terms is a different case and belongs to another state of things. If anyone is curious to see the two cases fully represented graphically, let him turn to Maxwell's Electricity and Magnetism, vol. i, fig. 5, Art. 143, for the first. and to vol. ii, fig. 15, Art. 434, for the second. Professor Nipher is at liberty to ascribe the second case, as his own, to the conditions about the sun, but there is no need to dedicate it to my theory, because it was not employed by me. There is no evidence that the sun is placed within a field of uniform force, produced outside of itself, nor is it necessary to resort to such a supposition in order to account for the forms of the coronal streamers, as seen from the earth.

When we come to consider the earth the case is different from that of the sun, since we have some reasons for thinking that the earth lies in a uniform field, generated by the action of the sun upon the earth. As I pointed out in the same paper it may be necessary to treat the field surrounding the earth by the full equation, written by Professor Nipher. I have published a theory for doing so, employing the configuration of the auroral streamers, if suitable observations can be obtained; [this Journal, February, 1891.]

3. It must be perfectly understood by students of this subject that most of the mathematical devices for discussing the field of force surrounding a polarized sphere, are physical fictions. There are many such devices, as for instance: (1) two equal masses of electricity with opposite signs placed infinitely near together at the center of the sphere, (2) the cosine distribution from a maximum superficial density at the axis, (3) the polarized sphere or the layers of gliding, (4) an indefinitely small magnet at the center of the sphere, and others. From such suppositions the mathematical treatment builds up the

observed lines of force surrounding the sphere. That there might be no doubt about my own position on the subject, I arrived at the formula for the lines of force by the geometrical and also by the harmonic analysis, never imagining that a critic would convert this common mathematical process into a physical reality of the sun's constitution. We are now interested to discover an analytical expression for the curvature of the coronal streamers; their physical nature is another question, one which is at present beyond our knowledge; it shares the perplexity attending any discussion of electrical or magnetic action. This then is the second irrelevant criticism. I did not suppose that the cosine distribution of electricity was actually plastered over the surface of the sun, and there was no need to allude to this point. Professor Nipher quotes me as saying, "on the supposition that we see a phenomenon *similar* to that of free electricity," (p. 21.) If the presence of the word "similar" was not sufficiently explicit as to my meaning, he should have quoted from page 19 of my paper as follows: "We have avoided speaking of the apparent coronal structure as a phenomenon of electricity, in deference to the doubt that free electricity can exist at such high temperatures on the sun's surface, but have shown that some force is present acting upon the corona according to the laws of electric potential." No one can at present explain the physical constitution of the matter that produces the coronal streamers, but I have in my work endeavored to show that they coincide in direction with stream lines produced by matter obeying the Law of the Newtonian Potential Function in the case of Repulsion. Such evidence as has been acquired to exhibit this identity of form between the coronal lines and the theoretical lines is now being published. [Proc. A. S. P., No. 16 or No. 17, and this Journal, July, 1891.]

Washington, April 29, 1891.

ART. LXII.—*On the recent Eruption of Kilauea;* by W. T. BRIGHAM.*

I PRESENT herewith my report on the changes that have taken place in the crater of Kilauea during the past month.

The last week of February, I found the crater in a state of intense but not extensive activity. During the seven months that had elapsed since my last visit, the depressed area of 1885 had been filled to a height of nearly 150 feet, and there were

* Report to Prof. Wm. D. Alexander, Surveyor General, dated Honolulu, H. I., April 8th, 1891.

indications that the entire floor of the crater, which has long been domed, had been elevated to some undetermined extent. One indication of this was seen in the great crack which extends across the trail near the eastern edge of the crater. This crack had closed nine inches.

The peaks that have long been an interesting feature of the fire area, as well as a land mark for the whole crater, had risen with the tide, and now towered at least 200 feet above the pools of liquid lava at their feet. Seen from the Volcano House, one-third of their total height was above the outer western wall. Their structure was loose, and so much smoke or sulphurous fumes escaped from almost their entire surface that it was not safe to attempt the ascent. On all sides they were surrounded by cones, generally hot and ejecting lava spatters. These cones were, in at least two cases, high up on the side of the main peaks, and exactly resembled the "Hornitos" of Humboldt. Several of the cones could be approached closely enough to throw blocks of lava into the oven door. One large cone of a group of three had become extinct, and one-half had fallen in fragments, showing the smooth inner walls, and the exceedingly superficial nature of its action. The wind was southerly, and it was therefore easy to go to the south and west of the fire area. Southeast of this was an extensive lava flow, covering some six acres, and proceeding from the base of a cone. To the southward of this a previous flow had formed a high ridge of "*a-a*"

West of the peaks was the most active portion, while in July the active lake was on the southeast side. The northern pool was the largest, of an irregular shape, having a promontory extending a third of its diameter from the middle of the western side. Its diameter, north and south, may have been 250 feet, and the banks were of unequal height but averaged fifteen feet above the lava surface. The next pool was the smallest, but the most active, and was 500 feet south of the first. Its diameter was less than one hundred feet, but the banks were overhanging. The third pool was near the last and intermediate in size.

All these pools were seemingly on a level, and were in my opinion connected; the crust intervening being not more than fifteen inches thick, and quite hot, although all the neighborhood was covered with a thick coating of "Pele's Hair," a good non-conductor. The usual intermittent action was no longer there: the surface had no time to cool, and no crust was allowed to cover the surface. From all the pools spatters of considerable volume were thrown on to the surrounding banks, and the direction of these jets was very peculiar. The molten lava was thrown obliquely, and the bright matter de-

scribed figures not unlike an interrogation point; the plane of these figures was quaquaversal.

There was remarkably little sulphurous vapor, and the absence of steam would have puzzled those geologists who impute to its agency the volcanic action. Outside of the crater, however, steam was issuing in several places; among these, the top of the wall near the Kau trail; the eastern wall between the two lateral craters; and the depressed wall between the main crater and Kilauea-iki.

As we left the pools in the evening, it was noticed that a cone some fifty yards west of the southern lake was sputtering in a very excited manner, and at 2 o'clock the next morning we saw it from the house spouting lava to an estimated height of twenty-five feet, while detached spatters were thrown twice that height. At 8 o'clock, when we left the crater, the fire fountain was still bright in the full morning light. It seemed to flow as freely as an uncapped artesian well.

This was nearly the condition of the crater a week later, when on March 6th, 1891, at 9.30 P. M. a slight earthquake was felt at the Volcano House, and the cones settled slightly. The next morning the peaks were out of sight. At Punaluu stronger earthquakes were felt, and at the Half Way House, the ground was in a continual tremor for some time; 300 shocks were counted in one night, but no accurate record was kept.

On the 2d of April, at your request, I visited the crater again and found the following condition of things. From the house the absence of two landmarks,—the peaks and the column of smoke, was at once noticed, and as night fell the accustomed look toward Halemaumau met not the slightest glimmer of light; all was as cold and dead as the grand old dome of Mauna Loa ten thousand feet above it.

The next morning the yawning pit was clear cut as seen from the house, and only a pale bluish smoke arose from its lips. Beyond, to the southward, was a white smoke that rose and fell, but was not of considerable extent. On descending into the crater the crack was found unchanged. Many smaller cracks intersected the trail, especially towards the middle of the crater, but the condition of the stone monuments on the Rock called the "Half Way House" showed conclusively that there had been very little disturbance in the crater itself: not one of the stone piles had been upset. The lava flow noticed on the previous visit was still warm, and on the borders of the depression was red hot. The entire fire area was gone. Peaks, cones and pools had vanished, and in their place was a pit

crater of elliptical outline, 2500 by 3000 feet, the major axis being nearly east and west. The walls were perpendicular and quite impassable. The estimated depth was 500 feet. There were many concentric and radial cracks making it dangerous in many places to approach the edge. Almost all the smoke proceeded from the hot upper crust of the border, none came from the bottom; and while every portion of the pit was clearly seen, the heated air constantly rising from the border made photographing a partial failure. Portions of the cracked lips had sunk, leaving steps toward the pit. There was a complete absence of any black in the walls or bottom; all shades of brown, red and yellow, but generally light: not in the least dismal or fresh looking, except for size, it looked quite like Mokuaweoweo, and might have been as old. The walls were in remarkably even layers; no cavities, dikes or great irregularities were to be seen. It was a wall of masonry whose cement time had crumbled, and it would hardly have seemed out of place had some vine trailed its festoons down the courses. The bottom was a confused mass of lava blocks of the same color as the walls, and was deeper at the west side. The impression was that the top of the peaks was there.

Owing to the bad arrangements of the Inter-Island S. N. Co. we were hurried away at daybreak the second morning, and so had no opportunity to photograph from the western wall, nor to take the desired measurements. The location is however settled with sufficient accuracy as the whole area covered by the last break-down and the pool to the eastward as well.

No word could be heard of any surface flow "*makai*" (seaward) of the crater, but from the steamer as we left Punaluu Saturday afternoon, a dense smoke was seen midway between Kilauea and the sea, which might have been a forest fire, or an outbreak.

It is useless to speculate as to the return of the fires: the present condition of the pit precludes any approach to them were the bottom dotted with fire-pools. In 1886, the wall was sloping on one side at least, affording access to the bottom. Any earthquakes may however topple down enough of the present wall to make a descent possible and the fires may be visible in a week or not for months. In its present condition Kilauea is most interesting to geologists, as in the walls of its included pit is an epitome of the formation of the mountain itself, a clean-cut section of 500 feet.

ART. LXIII.—*Turquois in Southwestern New Mexico;* by CHARLES H. SNOW.

SOME years ago excavations of ancient origin, the object of which was unknown until recently, were noticed in the Burro Mountains, southwest of Silver City, in Grant County, New Mexico. During the past year, Mr. W. J. Foley, then of Silver City, received a letter from a firm of Indian traders, in which it was stated that the Navajos claimed that turquois existed near Silver City, mines being described as having been worked at some remote period in an extensive, though primitive, manner. A search, thereupon instituted by Mr. Foley, resulted in an apparent connection between this story and the ruins above noted.

The locality has since been visited by the writer, who found the excavations situated upon several adjoining hill slopes. Although extending over considerable territory, they occur in isolated groups, and do not convey the impression of any very extended single effort. The size and character of the waste piles seem rather to indicate shallow works, although at one point a shaft or deep pit existed, and at another, close by an abruptly rising hill, either a shaft or a tunnel into the hill. All adits have long since been obliterated, and the piles of debris are more or less concealed by vegetation. The waste rock is in large pieces, and was evidently mined by the most primitive methods. It consists of gray quartz with white cleavable feldspar unlike that at the locality next described. Turquois was found in occasional small pieces attached to or seaming the rock, the color being the same pea-green shown by the "Los Cerillos stone." No metallic matter was noticed.

Continued search by Mr. Foley resulted in the location of another claim, about a mile distant from the one mentioned above, and near the "Chief of the Burros" copper mine, not now worked. There exists here a test pit, sunk some years ago as a copper exploration, the veins of turquois having been, strange to say, mistaken for veins of ores of that metal. The pit has a dimension each way of about three feet. It is upon the summit of a dike of a buff-colored rock traceable for some at each side of the pit. The dike matter, as seen in the pit and at one or more places upon the surface, contains a perfect network of turquois. The layers are from a line to an eighth of an inch in thickness, frequently starting in a thick layer, and then forking into several finer ones, which again subdivide. Where the layers are thick, a tendency of the rock to cleave away at the line of contact was sometimes, but not always,

noticed. The layers occasionally had an abrupt lateral termination, like the edge of a volume of thick fluid poured upon a flat surface.

Besides the layer formation, turquois was also noticed in small dots and in isolated patches, which occasionally differed from the vein matter in being of a more bluish shade. The prevailing color, however, was the green, common to the American turquois, and typified at Los Cerillos.

Turquois is known to occur at two other points in Grant County, New Mexico, both of them in the Burro Mountain district. From one of these the writer has specimens, consisting of sheets of very thin turquois having several square inches of surface area and from which gangue matter similar to that at the locality near the copper mine has been entirely removed by decomposition.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Compressibility of Hydrogen, Oxygen and Nitrogen.*—Amagat has subjected hydrogen, oxygen, nitrogen and air to pressures varying from one hundred to one thousand atmospheres, at temperatures of 0°, 100° and 200°. He finds that for hydrogen the values of dv/dt are practically independent of the temperature, the coefficient of expansion diminishing regularly as the pressure increases; while for nitrogen, oxygen and air, this coefficient passes through a maximum corresponding to the pressure at which the product pv has its minimum value. The values of dp/dt for hydrogen are also practically independent of the temperature, and nitrogen and air resemble hydrogen in this respect. Indeed the properties of hydrogen seem to be limiting values toward which those of all other gases tend; these limiting values of dv/dt and dp/dt being independent of the temperature, the former diminishing and the latter increasing as the pressure rises, the change being regular in both cases. At pressures up to 3000 atmospheres and at all temperatures, the isothermal lines have been shown by later experiments not to be exactly straight lines but to have a slight concavity toward the axis of abscissas. —*C. R.*, cxi, 871; *J. Chem. Soc.*, lx, 378, April, 1891. G. F. B.

2. *On Osmotic Pressure.*—In 1888, an analogy was pointed out by van't Hoff between the physical condition of a substance in dilute solutions and in the gaseous condition, osmotic pressure in the former case being the analogue of vapor-pressure in the latter. Boltzmann has now investigated osmotic pressure mathematically from the standpoint of the kinetic theory of gases. He supposes a cylinder, having a semi-permeable septum in the middle dividing it into two equal parts, and having its ends

closed by pistons. One of the compartments thus formed is filled with a dilute solution, the solvent of which alone can pass through the septum. The other compartment is filled with the solvent only. Osmose takes place, the solvent entering the solution through the septum and increasing the pressure exerted by that solution; the flow continuing until the osmotic pressure is balanced by the pressure within the vessel. By varying the pressures upon the pistons, equilibrium may be produced, and the osmotic pressure measured. The author proceeds to treat this osmotic system, with reference to the forces in operation in its various parts, in the same manner in which the problem would be discussed with reference to the kinetic theory of gases. And he finds in this way, that the resultant of all the forces in the two compartments which produce pressure on the septum, *i. e.*, the osmotic pressure, is equal to the gaseous pressure which the dissolved substance would exert were it distributed as a gas throughout the volume occupied by the solution, assuming that the mean kinetic energy of a dissolved molecule is equal to that of a gaseous molecule at the same temperature.— *Zeitschr. physik. Chem.*, vi, 474; *J. Chem. Soc.*, lx, 389, April, 1891. G. F. B.

3. *On the Production of Electrification in the Preparation of solid Carbon dioxide.*—In order to obtain rapidly and conveniently a considerable quantity of solid carbon dioxide, HAUSSKNECHT fastened over the jet of a wrought iron cylinder containing the liquid dioxide, such as is found in commerce, a bag of coarse linen cloth. By inclining the cylinder, the liquid issues under a pressure of 60–80 atmospheres, and in expanding into the gaseous state, absorbs so much heat as to solidify a portion of the escaping liquid, this solid collecting in the bag in the form of a compact snow. If the bag be made of strong canvas and have a capacity of from one to two liters, and if the experiments be conducted in the dark, it will be observed that the bag is filled with a greenish-violet light and that electric sparks from 10 to 20^{cm} long issue from its surface. If the hand be placed in these sparks the same peculiar prickling effect is noticed as when it is brought near the collector of an electric machine. This appearance of electrification is especially noticeable where imperfections occur in the compression pump or in the valves or manometers, so that the carbon dioxide issues under great pressure. The cause of this development of electrification is no doubt similar to or identical with that which operates in the hydro-electric machine of Armstrong. The liquid issuing with great force is converted into gas at once when it reaches the air. But under so great a pressure and at so low a temperature, the gas thus forced through the openings in the bag is accompanied by fine particles of the liquid, and probably of the solid also; the friction of these solid and liquid particles developing the electrification. For the success of this experiment it is essential that the carbon dioxide be absolutely free from air. The luminous phenomena in the interior of the bag appear first when the solid

dioxide has formed a layer of from half a centimeter to a centimeter in thickness. The author is continuing his investigations upon these phenomena.—*Ber. Berl. Chem. Ges.*, xxiv, 1031, April, 1891. G. F. B.

4. *On the Molecular Formula of Hydrogen Fluoride.*—PATERNO and PERATONER have determined the molecular mass of hydrogen fluoride by means of the lowering of the freezing point which it produces, the apparatus being the one ordinarily employed for this purpose, except that it was made entirely of platinum. The thermometric vessel was contained in a platinum tube, closed at its lower end and extending nearly to the bottom of the apparatus, being fastened to the cover. The space between the thermometric vessel and the walls of the tube was filled with mercury. After a series of comparative experiments proving this apparatus to give results coinciding with those obtained with a glass apparatus, it was applied to the determination of the molecular mass of hydrogen chloride in aqueous solutions of various strengths; and the results led uniformly to the formula HCl. On repeating the experiments with hydrogen fluoride however, the molecular mass obtained corresponded always to the doubled formula H_2F_2. Whether for more dilute solutions still this double molecule will split into two simple ones, farther experiments must determine. This result confirms the conclusion reached by Mallet by the vapor density method.—*Ber. Berl. Chem. Ges.*, xxiv, (Ref.) 298, April, 1891. G. F. B.

5. *On the Extraction of Oxygen from the Air.*—KASSNER has recently given further particulars concerning his process for extracting oxygen from the air on a commercial scale, based on the successive formation and decomposition of calcium plumbate. He now finds that in producing this plumbate, it is better to use a slight excess of calcium carbonate, say five per cent. Two molugrams (a molugram is the molecular mass in grams) of calcium carbonate, with this excess, is heated with one of lead oxide; and a resulting spongy product is obtained in this way, in which nearly the whole of the lead oxide is converted into the plumbate. The calcium carbonate is used in the form of limestone and it has been found unnecessary to stir the materials during the heating, which may be effected in an ordinary reverberatory furnace well supplied with air. To recover the oxygen the author heats the calcium plumbate in the presence of carbon dioxide, when the following reaction takes place:

$$Ca_2PbO_4 + (CO_2)_2 = (CaCO_3)_2 + PbO + O.$$

So that the process is a continuous one, the quantity of oxygen obtainable from a given quantity of charge being unlimited. Since the formation of the plumbate requires only a few minutes, and since its decomposition by the carbon dioxide is complete, the author thinks this process preferable to those of Boussingault and Brin; especially since the value of the material, the cost of the plant and the working expenses are extremely small.—*Dingl. J.*, cclxxviii, 468; *J. Chem. Soc.*, lx, 392, April, 1891. G. F. B.

6. *On Sodium-amine and Di-sod-ammonium chloride:*—JOANNIS has observed that at ordinary temperatures, sod-ammonium slowly decomposes into hydrogen and sodium-amine (sodamine); this decomposition tending toward a limit determined by the pressure of the hydrogen evolved. The sod-amine, NH_2Na, appears in small colorless transparent crystals; while that noticed by Gay Lussac was amorphous and of a blue or green color. The crystals dissolve in water with a hissing noise but without the evolution of gas. Disodium-ammonium chloride, NH_2Na_2Cl is an unstable compound which is obtained, mixed with sodium chloride, when sodium is treated with an excess of sodium chloride in presence of liquefied ammonia, insufficient in amount for complete solution of the salt. On washing with liquid ammonia this compound decomposes into sodium chloride which dissolves and into sodamine which is left. By water it is broken up into ammonia, sodium hydrate, and sodium chloride.—*C. R.*, cxii, 392; *Ber. Berl. Chem. Ges.*, xxiv, (Ref.) 292, April, 1891. G. F. B.

7. *Velocity of electrical waves in insulating fluids.*—Maxwell has shown that the relation $n^2=\mu$ is a consequence of his electromagnetic theory of light. In this formula n is the rate of the velocity of the wave in a vacuum to that in the substance examined and μ is its dielectric constant. L. ARONS and H. RUBENS, employing Hertz's method of studying electromagnetic waves, have examined the relation given by Maxwell and find it very nearly fulfilled in the case of the four fluids investigated by them.—*Ann. der Physik und Chemie*, No. 4, 1891, pp. 581–592. J. T.

8. *The telephone in an optical apparatus for measurement of electrical currents.*—MAX WIEN employs the movement of the telephone diaphragm to measure both constant and alternating currents and prefers this instrument to the usual form of dynamometer. The iron diaphragm of the telephone is replaced by a thin metallic plate similar to those used in aneroid barometers. A piece of iron is placed upon this diaphragm opposite the pole of the telephone magnet and the movement of the diaphragm is communicated by a simple lever arrangement to a small mirror, which deflects the excursions of a spot of light into the field of view of a telescope. The amplitude of the movement of the spot of light is a measure of the current strength. The author discusses the accuracy of the indications and shows that quantitative results can be obtained by this simple instrument.—*Ann. der Physik und Chemie*, No. 4, 1891, pp. 593–623. J. T.

9. *Photography of the ultra red rays.*—In a communication to the Royal Society, March 12, Mr. GEORGE HIGGS stated that the alzarine blue S possesses in a high degree sensitive giving properties for rays throughout the region comprised between wavelengths 6200 and 8000, and does not like cyanin lower the sensitiveness to the violet and ultra violet. The method of preparation of the alzarine was described. With a slit 1/1000 inch in width and an exposure of forty minutes results have been obtained in the

region of great A of the second order, which possess all the detail and definition usually so characteristic of the violet end.—*Nature*, April 2, 1891, p. 525. J. T.

10. *Lecture experiment on magnetic screening of conducting media.*—J. J. BORGMAN describes the following experiment. A Lecher's tube (Ann. der Physik und Chemie, xli, p. 850) is put by means of two cork rings into another large (4 cm. diam.) glass tube, with a crane on one end. Holding the tube in one hand, and approaching it to a wire in which electrical waves are produced, a continuous lighting of the tube is seen. If the outer tube is filled with dilute sulphuric acid the light disappears. This is not the case when the outer tube is filled with water.—*Nature*, April 23, 1891. J. T.

II. GEOLOGY AND NATURAL HISTORY.

1. *Eruption of Kilauea.*—The Daily Pacific Commercial Advertiser of April 30th reports the following additional facts: "The breakdown is slightly larger than the one of 1886. It also differs from the one of 1886 in the following respects: in 1886 the fire appeared to have entirely gone out, there seemed to be little steam left, and for three months the crater was absolutely dead and cold, with the exception of the still warm lava which had run out prior to the breakdown. The lava then came back slowly, and it was considerably over a year before the whole basin filled up again. The breakdown of 1891 left hot lava still to be seen in the cracks around the edge of the breakdown, and dense clouds of vapors, steam and intense heat arising at several points from the bank. After a lapse of only three weeks the molten lava again appeared in the pit, and it is now filling up rapidly. The news brought by the steamer W. G. Hall was that up to the 26th inst. the bottom of the pit had filled up about 100 feet, and a lake of liquid lava formed some 250 to 300 feet in diameter. This is the result of only ten days' action. The bottom of the pit was steadily rising and the size of the lake increasing, and activity showing itself at new points every day. The illumination was very bright, being visible at night at Punaluu, thirty miles away."

2. *Geological Survey of Ohio.*—First Annual Report under the third organization by Edward Orton, State Geologist. 330 pp. 8vo.—This very valuable report, chiefly by Professor Orton, treats of the origin and accumulation of mineral oil and natural gas, and of the Trenton and Clinton limestone and other rocks in Ohio, as sources of these materials. The chapters on these topics are preceded by one on the general geological structure of Ohio. The report is accompanied by two maps of the oil fields and gas fields. It closes with a chapter on the measurement of natural gas in gas wells, pipe lines, etc., by S. W. Robinson.

3. *Iron Ores of Minnesota by* N. H. WINCHELL and H. V. WINCHELL. Bulletin No. 6 of the Geological Survey of Minnesota, 420 pp. 8vo. With a geological map, 44 plates and 26

illustrations in the text. Minneapolis, 1891.—This volume treats, as the title-page further states, of the geology, discovery, development, qualities and origin of the ores, and of comparison with those of other iron districts. The rocks are described with some detail, colored microscopic sections given of several of them, and the age of the deposits is discussed at length. One of the interesting plates of the volume represents the famous Greenland mass of iron found embedded in basalt and weighing 19 tons, now in the museum of the Royal Academy at Stockholm.

4. *The Tertiary Insects of North America;* by SAMUEL H. SCUDDER, U. S. Geol. Surv. of the Territories, F. V. Hayden. Vol. xiii, pp. 734, Plates I–XXVIII. Washington, 1890.—This volume brings the subject of fossil Tertiary insects into prominence as a department of American paleontology. Formerly, it was impossible to make any general comparison between the American and European faunas, as the former was meager both in specimens and species. Owing to the rapid geological exploration of the West, and to the labors of the author of this monograph, the lack of material and of definite knowledge have both been removed. Moreover, as the insect-bearing rocks are so extensive, and have been investigated at so few localities, it is evident that further researches will result in a richer and more varied fauna than has yet been developed elsewhere.

Six hundred and twelve species are described, divided among the orders as follows: Myriapoda 1, Arachnida 34, Neuroptera 66, Orthoptera 30, Hemiptera 266, Coleoptera 112, Diptera 79 Lepidoptera 1, and Hymenoptera 23. By far the most abundant fauna occurs in the Tertiary lake basin at Florissant, Colorado. Some of the higher orders of insects are more fully represented than is indicated in the enumeration of species. Their description is reserved for the acquisition of more and better material. Other localities yielding fossil insects and included in the volume are: Green river, Fossil and Horse Creek, Wyoming; the vicinity of Quesnel, British Columbia; Scarboro, Ontario; and Port Kennedy, Pennsylvania. C. E. B.

5. *Trilobites of the Upper Carboniferous of Kansas.*—The Kansas City Scientist—a popular scientific monthly of 16 pages 8vo, made the official organ of the Kansas City Academy of Science—contains, in its March number, an article by S. G. Hare on species of Phillipsia, illustrated by a plate; and the February number contains an account of foot-prints from the Upper Carboniferous, by E. Butts.

6. *An Introduction to the study of Petrology: The Igneous Rocks;* by FREDERICK H. HATCH. 128 pp. London and New York, 1891 (Swan Sonnenschein & Co.; Macmillan & Co.) This little book will be found useful by those desiring a concise account of the minerals which are present in the various types of igneous rocks and of the composition and occurrence of these rocks themselves. The space is about equally divided between these two parts, and the descriptions are probably as satisfactory as is possible where the subjects are treated with such brevity.

7. *Sinopsis Mineralógica ò Catálogo descriptivo de los Minerales por* CARLOS F. DE LANDERO. pp. 1–432. México, 1888. This is an alphabetical list of the various mineral species, giving brief descriptions with also numerous synonyms. It will be useful not only at home but wherever a knowledge of the Spanish and local Mexican names of minerals is needed.

8. *New Meteorites.*—Mr. Edwin E. Howell gives descriptions of a number of new meteorites in vol. i of the Proceedings of the Rochester Academy of Science, illustrated by figures (part of which have been used in this Journal, vol. xl, p. 223). They are named the Welland Meteorite, from Welland, Ontario, Canada; the Hamilton County, from Texas; the Puquios, from Copiapo, Chili; the De Cewsville, from Ontario, Canada; two from Atacama, Chili, called the Doña Inez and the Llano del Inca; and from Chili three others, the El Chañaralino, la Primitiva and the Calderilla. The Hamilton County meteorite weighs 179 lbs., and its largest diameter is 17½ inches. A fine plate printed from the iron exhibits grandly the Widmanstätten figures.

9. *Die Protoplasmaverbindungen zwischen benachbarten Gewebeselementen in der Pflanze;* by F. KIENITZ-GERLOFF. (Bot. Zeit., 1891, Nrn. 1–5, Taf. I–II).—The continuity of protoplasm in adjacent cells of vegetable tissue has since its discovery been a subject of the greatest interest and significance. Through this new and unexpected feature of plant-anatomy it has been hoped that light might be thrown upon a host of physiological processes hitherto unexplained; and the importance thus attached to the histological fact has very naturally made the subject an alluring one for original investigations. Perhaps, indeed, no point of plant-anatomy has so often, within the last few years, been chosen as a subject for special study, nor in most cases proved so barren of new results. Since the appearance of Gardiner's papers a number of new instances of the phenomenon in question have, it is true, been observed and recorded. Few details, however, have been added to our knowledge of individual cases, nor have the methods employed in the treatment and staining of preparations been essentially improved. Russow's highly interesting hypothesis that the threads uniting the protoplasm in adjoining cells arise from the delicate fibrillæ observed between the nuclei in cell-division, has been neither confirmed nor refuted; and in regard to the physiological significance of the continuity of protoplasm theories are as conflicting as ever. The present paper by Kienitz-Gerloff, treating the subject both in its anatomical and physiological aspects, is therefore especially welcome.

After a brief historical sketch the author proceeds to consider the different ways of treating sections to bring out clearly the connecting threads of protoplasm, and states that he has met with the best success by placing sections of fresh material in a solution of potassic-iodide to fix the protoplasm with but little contraction, before such reagents as sulphuric acid or chloriodide of zinc are employed to act upon the cell-walls. This is

Terletzki's modification of the earlier methods. In the case of succulent plants it was found advantageous to dip the parts to be sectioned in boiling water and then harden them in absolute alcohol. As a coloring agent Hoffmann's aniline blue was chiefly employed, but the combination of this staining agent with picric acid, so highly recommended by Gardiner, was found to give too faint a tint—an experience we believe, which has been shared by others. Especial difficulty was found in coloring, where the cell-walls were cutinized, and in such cases a strong solution of methyl-violet gave the most satisfactory results. As a mounting agent glycerine proved useless, while Damar and Canada balsam are recommended.

In a systematically arranged list the author enumerates some sixty species, from the *Hepaticæ* upward, which have been investigated by him, and indicates in each case the elements between which protoplasmic threads were observed. As the tissues in and between which the continuity of protoplasm has been detected embrace nearly every kind of histological element, the conclusion—already stated by others upon a less secure basis of observation—is drawn that all the elements in the higher plants are so connected. A single exception, however, is made in the case of the guard-cells of the stomata, the walls of which according to Kienitz-Gerloff are entirely free from perforating threads.

The morphology and origin of the threads are excellently discussed and the phenomena observed are well illustrated in a number of figures. Details of course cannot be given here. It may be mentioned that the spindle-shaped enlargements of the protoplasmic threads, so frequently observed near their middle but not altogether confined to that region, are plausibly explained on the ground that some of the lamellæ of the cell-walls, notably those near the middle, are much less strongly swollen than others by such reagents as sulphuric acid, and therefore exert less pressure upon the penetrating threads. The protoplasm accordingly remains in greater quantity at these points. Our attention is also called to the fact that the connecting threads as they exist in nature, are doubtless much larger than they appear after treatment with reagents employed to bring them into view. Both the morphological descriptions and accompanying figures render Russow's theory, just mentioned, very doubtful. Its disproof however is not altogether conclusive.

Of especial interest is the treatment of the physiological aspect of the continuity of protoplasm, and here Kienitz-Gerloff favors strongly the theory of Kohl and Wortmann, that a transference of protoplasm and the substances it contains really takes place from cell to cell by means of the threads. This view has been opposed by various observers, most recently by Noll and Zimmermann, who are inclined to consider the function of the continuity rather the communication of shock or impulse. Of the several very plausible reasons which Kienitz-Gerloff gives for believing that a transference of matter is effected by the structures in

question we may only mention a curious negative argument from the guard-cells of stomata. In the autumn, as is well-known, the organic contents of the cells in deciduous leaves retire in great part into the stem, but in the guard-cells alone the protoplasm remains in tact even after the fall of the leaf. As we have just seen, these are the only cells which possess no protoplasmic connection with the other elements, and the inference is easy that this is why their contents are not withdrawn. Reasoning from the converse it appears probable that the connecting threads among the other elements are the structures active in removing the organic substances from cell to cell, and finally out of the leaf into the stem. These facts taken alone would have but little weight but in conjunction with various other phenomena furnish a particularly interesting bit of evidence. The article closes with an excellent bibliography of the subject. B. L. R.

10. *Protoplasmaverbindungen bei Algen;* (Berichte der deutsch. bot. Gesellsch., ix, pp. 9–16).—In a paper of this title Dr. F. G. Kohl describes a series of observations upon the continuity of protoplasm in various cryptogams ranging from the *Conjugatæ* to the ferns, thus neatly supplementing the work of Kienitz-Gerloff just discussed. Kohl's methods of bringing the connecting threads to view are very interesting, since they are novel as applied for this purpose. Instead of using some reagent to act upon the cell-wall, he produces a slow plasmolysis, employing a solution of tannin-anilin (as recommended by Loeffler to show the cilia of bacteria), and then, after staining the preparations, employs dilute glycerine to remove the coloring matter from the cell-walls. The well-known *Spirogyra*, which has been studied from so many different points of view, is once more made to do service as a typical example, and the continuity of the protoplasm in adjoining cells of its filaments is described and figured in detail. Kohl states further that he has observed similar phenomena in *Cladophora*, *Ulothrix* and other related forms. While in some of the algæ, notably in certain *Florideæ* the continuity of the protoplasm can be very readily demonstrated,—indeed it was here that it was first observed—there has been considerable doubt as to the extent to which the cells of the *Fucoideæ* are thus connected. As examples of the latter group Kohl studied *Himanthalea lorea* and several species of *Fucus*. He states that by the use of the method just described he has been able to demonstrate a general continuity of protoplasm between the various cells of these plants; and that the phenomenon is by no means confined, as some have supposed, to the so-called sieve-hyphæ.

B. L. R.

III. Miscellaneous Scientific Intelligence.

1. *The Journal of Comparative Neurology.* A Quarterly Periodical devoted to the Comparative Study of the Nervous System. Edited by C. L. Herrick, Professor Biol. Univ. Cincinnati. 106 and xviii pages, 8vo. Cincinnati, Ohio.—This first

number of this new quarterly shows capacity in the editor to do well his part in conducting it. The first two papers, covering 35 pages, are by Prof. Herrick: the first, "Contributions to the comparative morphology of the central nervous system," with plates I–IV, and the second, "Topography and histology of the brain of certain reptiles, with plates IX–X. Another article, over 50 pages in length, by C. L. Twines, treats of the morphology of the Avian brain, and is illustrated by plates V to VIII. In addition there are many notes from other journals, and a list of new publications, besides a statement by the editor of "the problems of comparative neurology," setting forth the range of especially important subjects, which it is the object of the journal to elucidate. On account of the high character of the journal and the great importance of the field it covers, it deserves liberal support.

2. *A Journal of American Ethnology and Archæology.* Editor, J. WALTER FEWKES. Vol. I, 132 pp. Boston and New York, 1891 (Houghton, Mifflin & Co.).—The first volume of this new journal gives gratifying promise as to the interest and value of the series which it commences. The leading article by the editor, Mr. Fewkes, is upon some summer ceremonials at Zuñi Pueblo, giving results obtained in connection with the Hemenway Southwestern Archæological Expedition. It is liberally illustrated, and gives a very interesting account of some of the dances and other ceremonies of this curious people. A second paper is on Zuñi melodies by Benjamin Ives Gilman, accompanied by musical scores. A third paper, also by the editor, is on a reconnoisance of ruins in or near the Zuñi reservation, with maps and other illustrations. The publishers have made the appearance of the volume very attractive. (Price two dollars).

3. *Helmholtz Celebration and Medal.*—Steps are being taken to celebrate the seventieth birthday of Professor von Helmholtz, which occurs on August 31st. A marble bust of Professor Helmholtz is being made which will be presented to him on that occasion, and a fund is being raised the income of which is to be applied, primarily, to the bestowal of a *Helmholtz medal* on eminent investigators of all nations in the fields of Professor Helmholtz's activity. An international committee, which has been formed to carry out these schemes, solicits contributions, which may be sent to the committee's bankers, Mendelssohn & Co., Berlin. Professor Henry P. Bowditch of Harvard University will forward the contributions of such as may find it more convenient to send to him, with the names of the contributors, to the bankers appointed by the committee. We understand also that an especial movement has been started among ophthalmologists and otologists of this country and Canada, whose contributions are received and forwarded by Dr. Herman Knapp of New York. All contributions should be sent as soon as possible.

4. *National Academy of Sciences.*—The following is a list of papers accepted for reading at the meeting held at Washington, April 21–24:

A. S. PACKARD: Further studies on the brain of Limulus Polyphemus.

S. P. LANGLEY: On aerodromics.

F. H. BIGELOW: The Solar Corona, an instance of the Newtonian potential in the case of repulsion.

J. S. BILLINGS: Report on the human bones of the Hemingway collection in the U. S. Army Medical Museum, prepared by Dr. Washington Matthews, U. S. A.

A. A. MICHELSON: Application of interference methods to spectroscopic measurements.

H. S. PRITCHETT: The Corona from photographs of the eclipse of Jan. 1, 1889.

LEWIS BOSS: Stellar motion problems.

IRA REMSEN: Effect of pressure and temperature on the decomposition of diazo-compounds. Researches on the double halides.

M. CAREY LEA: Allotropic silver; note on a paper by M. G. Lippmann.

H. A. ROWLAND: On the yttrium earths, and a method of making pure yttrium.

E. D. COPE: On the distribution of colors in certain North American reptiles.

THEO. GILL: The taxonomy of the apodal fishes.

W. K BROOKS and E. G. CONKLIN: Researches on the embryology of mollusks.

Report of the Watson Trustees, and Presentation of the Watson Medal to Prof. Arthur Auwers of Berlin.

5. *Magnetite Ore Districts of Brazil—Erratum.*—Dr. O. A. Derby informs the Editors that the mineral occurring with the magnetite at Ipanema and referred to on page 316 of the April number as enstatite, has proved on further examination of better material to be barite. One specimen shows free crystals of barite upon its surface.

Examen Quimico y Bacteriologico de las Aguas Potables por A. E. Salazar y C. Newman; con un capitulo del Dr. Rafael Blanchard. 513 pp. 8vo, with seven plates. London, 1890.

Leçons sur les Métaux, Professées a la Faculté des Sciences de Paris, par Alfred Ditte, Professeur de Chimie à la Faculté. Premier Fascicule, Paris, 1891.

The number system of Algebra treated theoretically and historically by Henry B. Fine, Ph.D. 131 pp. 12mo. Boston and New York, 1891.

Report of the Superintendent of the U. S. Coast and Geodetic Survey for the year ending June, 1888, pp. 566. 4to, Washington, 1889.

Determinations of Latitude and Gravity for the Hawaiian Government, by E. D. Preston, pp. 563. 4to, Washington, 1890. (United States Coast and Geodetic Survey. Appendix 14.)

The International Astrophotographic Congress and a Visit to Certain European Observations and other Institutions, by Albert G. Winterhalter, pp. 354. 4to, Washington, 1889. (Washington Observations, 1885, Appendix I.)

Magnetic Observations at the United States Naval Observatory, 1888 and 1889, by Ensign J. A. Hoogewerff, U. S. Navy, pp. 100. 4to, Washington, 1890, (Washington Observations, 1886, Appendix I.)

Saturn and its Ring, 1875–1889, by Asaph Hall, Prof. of Mathematics, U. S. Navy. pp. 22. 4to, Washington, 1889. (Washington Observations, 1885, Appendix II.)

Brachiopoden der Alpinen Trias von A. Bittner, pp. 325. 4to, Wien, 1890. (Alfred Hölder, Vienna Academy.)

OBITUARY.

JOSEPH LEIDY.—Dr. Joseph Leidy, the eminent Comparative Anatomist, Zoologist and Paleontologist, died at Philadelphia on the 30th of April. He was born in the same city on the 9th of September, 1823. His father was a native of Montgomery County, Pa., but his ancestors on both sides were Germans from the valley of the Rhine. While yet a school-boy, minerals and plants were eagerly collected and studied, and also anatomical dissections were begun, a barnyard fowl being the first subject. He entered the Medical School of the University of Pennsylvania in 1840 and devoted his first year to practical anatomy. Having taken his medical degree in 1844, he became the next year, then 21 years of age, Prosector to Dr. Horner, Professor of Anatomy in the university ; and at the death of Dr. Horner, in 1853, he was appointed his successor.

In 1844 he made the many remarkable dissections of terrestrial mollusks, the drawings of which cover sixteen plates and illustrate thirty-eight species in Dr. Binney's fine work on the Terrestrial Mollusks of the United States—showing in all not only remarkable power as an anatomist entitling him to high rank, as Dr. Binney remarks, among philosophical zoologists, but also great skill as a draftsman. Thus, from the first, Dr. Leidy was the thorough, minutely accurate and untiring investigator.

After the publication of Dr. Binney's work in 1845, he was elected a member of the Academy of Natural Sciences of Philadelphia ; and from that time he was its most active member, hardly a volume of its publications appearing without one or more papers on the results of his researches.

Dr. Leidy's contributions to Zoology and Comparative Anatomy have a wide range. The Lower Invertebrates occupied a large share of his time. Besides multitudes of short papers, he published in 1853, a work of 67 pages, illustrated by ten plates, on "A Flora and Fauna within Living Animals"—of the botanical part of which Dr. Gray said in this Journal—"a contribution of the highest order, the plates unsurpassed if not unequalled by anything before published in the country." In 1879 appeared his large quarto volume on the fresh-water Rhizopods of North America, containing 48 colored plates, the material of which was in part collected during two seasons in the Rocky Mountain region under the auspices of the Hayden Exploring Expedition. As a portraiture of the Doctor over the little memberless species, we quote from his concluding remarks : "The objects of my work have appeared to me so beautiful, as represented in the illustrations, and so interesting as indicated in their history which forms the accompanying text, that I am led to hope the work may be an incentive, especially to my young countrymen to enter into similar pursuits. 'Going fishing?' How often the question has been asked by acquaintances as they have met me, with rod and basket, on an excursion after mate-

rials for microscopic study. 'Yes,' has been the invariable answer, for it saved much detention and explanation; and, now, behold, I offer them the result of that fishing. No fish for the stomach, but as the old French microscopist, Joblet, observed, 'some of the most remarkable fishes that have been seen,' and food fishes for the intellect." He delighted in his work because he knew that there was no fact in connection with the structure and functions of the simplest of living things that was not profound and comprehensive, that did not reach up through all species to the highest.

The Vertebrates described by him were mainly fossil species. Dr. Leidy has the honor of having opened to geological science a general knowledge of the remarkable mammalian fauna of the country, and especially that of the Rocky Mountain region. Species had been before described, but through him the general range of North American species began to be known. In 1847, he published on the fossil Horse; in 1850, on the extinct species of the American Ox; in 1852, and 1854 on the extinct Mammalia and Chelonia from Nebraska Territory, collected during the survey under Dr. D. D. Owen; in 1855, on the extinct Sloth tribe of North America; in 1869, on the extinct Mammalian fauna of Dakota and Nebraska, a thick quarto volume published by the Philadelphia Academy of Sciences, based on materials that had been gradually and continuously accumulating for the last twenty years; and in 1873 contributions to the extinct Fauna of the Western Territories, making the first quarto volume of the Hayden Survey. The last two works mentioned contain over 800 pages of text and nearly 70 of plates. Besides these large works numerous short papers from time to time appeared.

Dr. Leidy retired from this particular field when questions of priority began to start up, it being no part of his nature to quarrel, and having the firm belief, as he said, that the future would award credit where it was deserved. His work among the fossil Vertebrates extended also to Fishes, Batrachians and Reptiles of different geological periods, and among his contributions, that on the Reptiles of the Cretaceous period of 1865, published by the Smithsonian Institution, covers 136 pages and is illustrated by 20 plates.

Dr. Leidy's zeal never flagged; his labors came to an end only with his sudden death. Eight days before, he delivered his last University lecture. Beginning original work before he was twenty, his published papers and larger books continued to appear through half a century and number over nine hundred. As is well said in one of the many tributes to him published in the Philadelphia papers after his decease:

"He possessed to the end of a long career the freshest capacity of seeing the opportunities and openings for discovery and research offered by familiar phenomena. His vast store of exact and diverse knowledge in the whole wide field of animate nature was under the command of a logical judgment and synthetic

powers which saved him from vagaries. These high intellectual powers were served by an untiring capacity for work and equal skill of eye and hand.

"These are rare gifts; but they are none of them, nor all of them put together, as rare as his character. His simplicity, his transparent sincerity, his ingenuous anxiety to serve science and to serve science alone, his freedom from all desire for the rewards, the honors and the recognition after which lesser men go a-wandering, were as remarkable as his scientific powers."

Never were words more truthful. Honors came to him from all parts of the civilized world, and more because unsought.

Dr. Leidy leaves a wife and an adopted daughter.

JOHN LECONTE, Professor of Physics and Industrial Mechanics in the State University at Berkeley, California, died on the 29th of April, aged 72 years. Professor LeConte, the oldest son of the naturalist, Lewis LeConte, was born in 1818 in Liberty County, Georgia. He was graduated from Franklin College, now the University of Athens, Georgia, when he was twenty years old, and from the College of Physicians and Surgeons in New York City three years later. He then entered upon the practice of medicine at Savannah where he remained for four years. In 1846 he was called to the Chair of Natural Philosophy in Franklin College, which he occupied until 1855. In the following year he lectured on chemistry at the College of Physicians and Surgeons in New York City; in the same year he was made Professor of Natural and Mechanical Philosophy in South Carolina College at Columbia, S. C., a position which he held for thirteen years.

In 1869 he was appointed Professor of Physics and Industrial Mechanics in the University of California, and this position he retained until 1881. From 1876 to 1881 he held, in connection with his professorship, the office of President of the university, and at the expiration of that term he retired to the Chair of Physics, which he occupied until the time of his death.

Professor LeConte's energies were early devoted to medicine but later he turned toward physical science and in both departments he made numerous contributions which have been published in the Proceedings of the American Association and in various scientific journals. Among those which have appeared in this Journal may be mentioned papers on the influence of musical sounds on gas jets; on the influence of solar light upon combustion; physical studies on the waters of Lake Tahoe; several papers upon various aspects of the phenomena of capillarity; also on sound shadows in water. In 1857 he delivered a course of lectures on "The Physics of Meteorology" before the Smithsonian Institution, in Washington, and in 1867 he read an important paper on "The Stellar Universe" before the Peabody Institute, in Baltimore. His whole list of published writings includes about a hundred papers extending over a wide range of subjects.

In 1879 he received the degree of LL.D. from the University of Georgia and the same year he was made a member of the National Academy of Sciences. A younger brother, Professor Joseph LeConte, also of the University of California, and closely associated with him through life, is well known as a geologist and physiologist.

Julius Erasmus Hilgard, late Superintendent of the United States Coast Survey, died at his home in Washington on the 8th of May, after a long and painful illness. He was the son of Theodore Erasmus Hilgard, an eminent German jurist, and was born in Zweibrücken, Bavaria, Jan. 7, 1825. He came to this country when ten years of age, and until 1843 resided in Belleville, Ill. In that year he removed to Philadelphia, where he took up the study of civil engineering, and two years later he became one of the assistants of Professor Bache on the Coast Survey. In 1862 he was promoted to the position of assistant in charge of the Coast Survey Office. This position he held until 1881, when, upon the death of Captain Patterson, he was appointed Superintendent; increasing physical disability, however, interfered with the discharge of his duties and finally led to his resignation, which took effect in 1886.

Dr. Hilgard's active labors, for nearly forty years, were chiefly in connection with the development and administrative work of the Survey, and here he did very important and valuable service to science and to the country. He had charge of the construction and verification of the standards of weights and measures, and was for some time engaged in preparing metric standards for distribution to the several States. He was also engaged in researches and the discussion of the results in geodesy and in terrestrial physics and in perfecting methods and instrumental means connected with them. One of the most important pieces of work with which he was connected was the determination of transatlantic longitude in 1872; a result of this was to establish an important correction to the longitude of Paris as reckoned from Greenwich. A chart compiled by him giving the magnetic declination over the United States for 1875 was issued in connection with the Coast Survey Report, and, also with a descriptive article in this Journal (xix, 173).

Professor Hilgard was one of the original members of the National Academy of Sciences, and served for years as its Home Secretary. He was made President of the American Association for the Advancement of Science in 1874.

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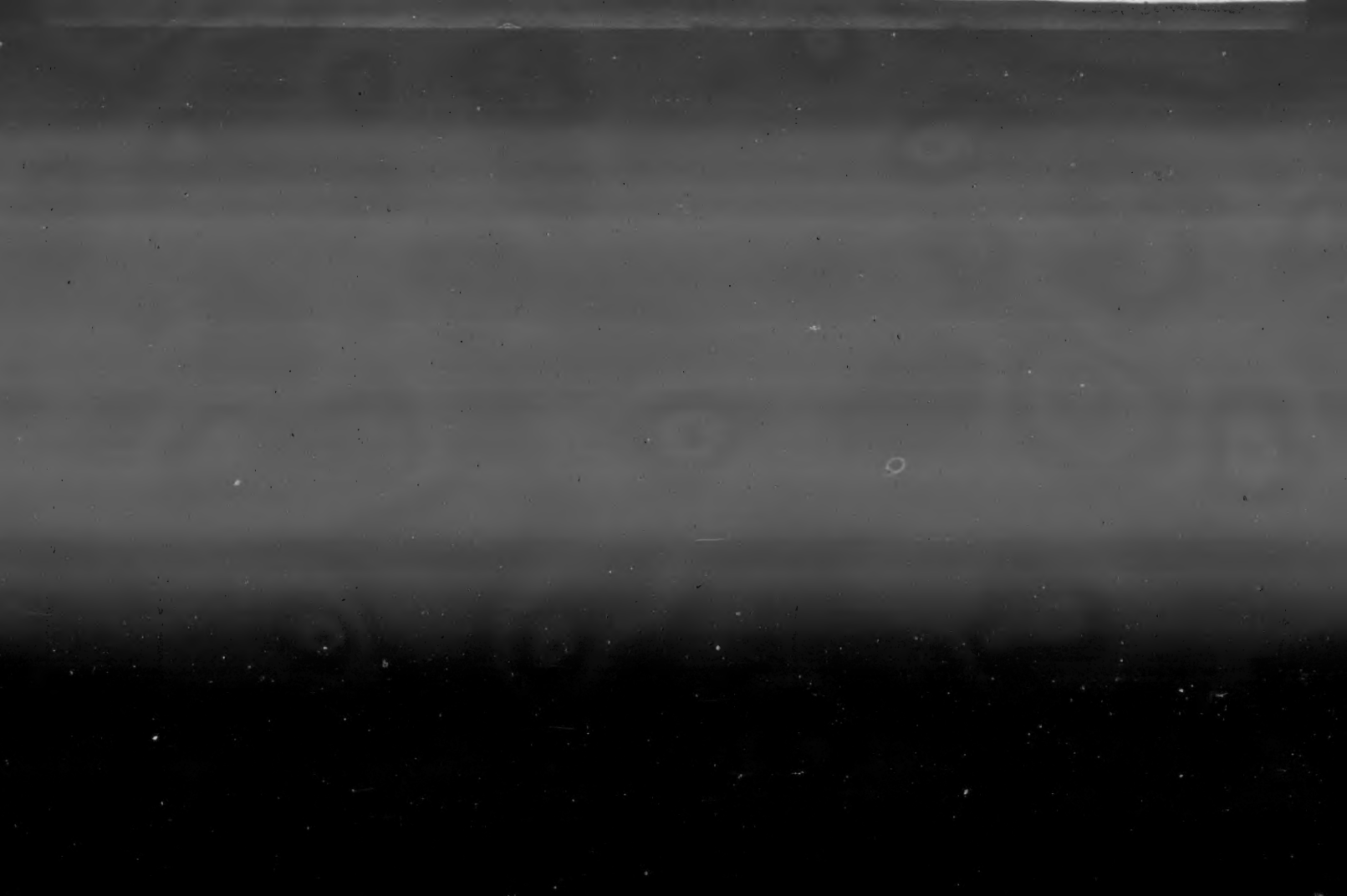

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